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# THREE CENTURIES OF CHEMISTRY







FROM THE WORKS OF JOHN WILKINS.



# THREE CENTURIES OF CHEMISTRY

*Phases in the Growth of a Science*

BY

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## INTRODUCTORY NOTE

IN writing this short story of a great subject, the author has tried to balance himself simultaneously upon the two stools of the philosophic historian and of the scientific chemist; for he has chosen to portray the genesis and evolution of ideas rather than to rehearse every discovery that gains a place in our chemical text-books.

The intention has been, first to show the very broad foundations that were provided to sustain the present structure of science, and to what inspirations of what manner of men and circumstances we owe the laying and the consolidation of those bases (Chapters I-IV). Next, to show in action the application of this process to chemistry, and the issue of a dominant motive which has given continuity to the whole subsequent trend of the science (Chapter V). The ensuing phase in chemistry displays the long struggle between the new mode of thought and the ancient philosophy, ending with the eighteenth century in a solution of the central problem and the final ousting of mediævalism (Chapters VI-X). In the next phase, the key-problem has taken on a deeper meaning, and the arrival at its successful interpretation is set forth as we know it to-day; while note is taken of the oncoming of still another era (Chapter XI).

Two sorts of reader are addressed: on the one part those who, like the writer himself, have first become students of chemistry and have then wished to recognize their heirship, and to appreciate the philosophy of their subject; on the other part those who do not understand the technicalities of chemistry and physics, but are interested either in the evolution of modern thought or in applied logic. With this in view, the endeavour has been to give special notice to the sequence and the recurrence of phases which are probably common to the growth of every science, *mutatis mutandis*. Chemistry is just old enough to have completed two cycles, of which the first occupied ages and the second a century; and it may interest some students to trace broad parallels in

the careers of other systems of knowledge, either older or newer.

It remains only to caution non-chemical readers that in the parts of the book which exhibit recent developments (Chapter XI), one wing of chemistry has been left almost undepicted. Organic chemistry is perhaps the most wonderfully elaborated section of the science; but its inferences depend upon specialized information and nuances of qualitative behaviour which only trained chemists can appreciate, and consequently there is here discussed only its general bearing upon the theme which the writer sees as the dominant intellectual aim of the physical sciences.

A postscript to the book contains observations on chemistry as a profession. Parts of the material included in Chapters III and IV had already been epitomized in *Nature*, 1924, and in the Royal Society's Handbook to the Wembley exhibit of that year. Considerable portions of Chapter XI comprise sections of the writer's contribution on physical chemistry in *Chemistry in the Twentieth Century* (Benn, 1924).

I. M.

Durham,  
October 1925



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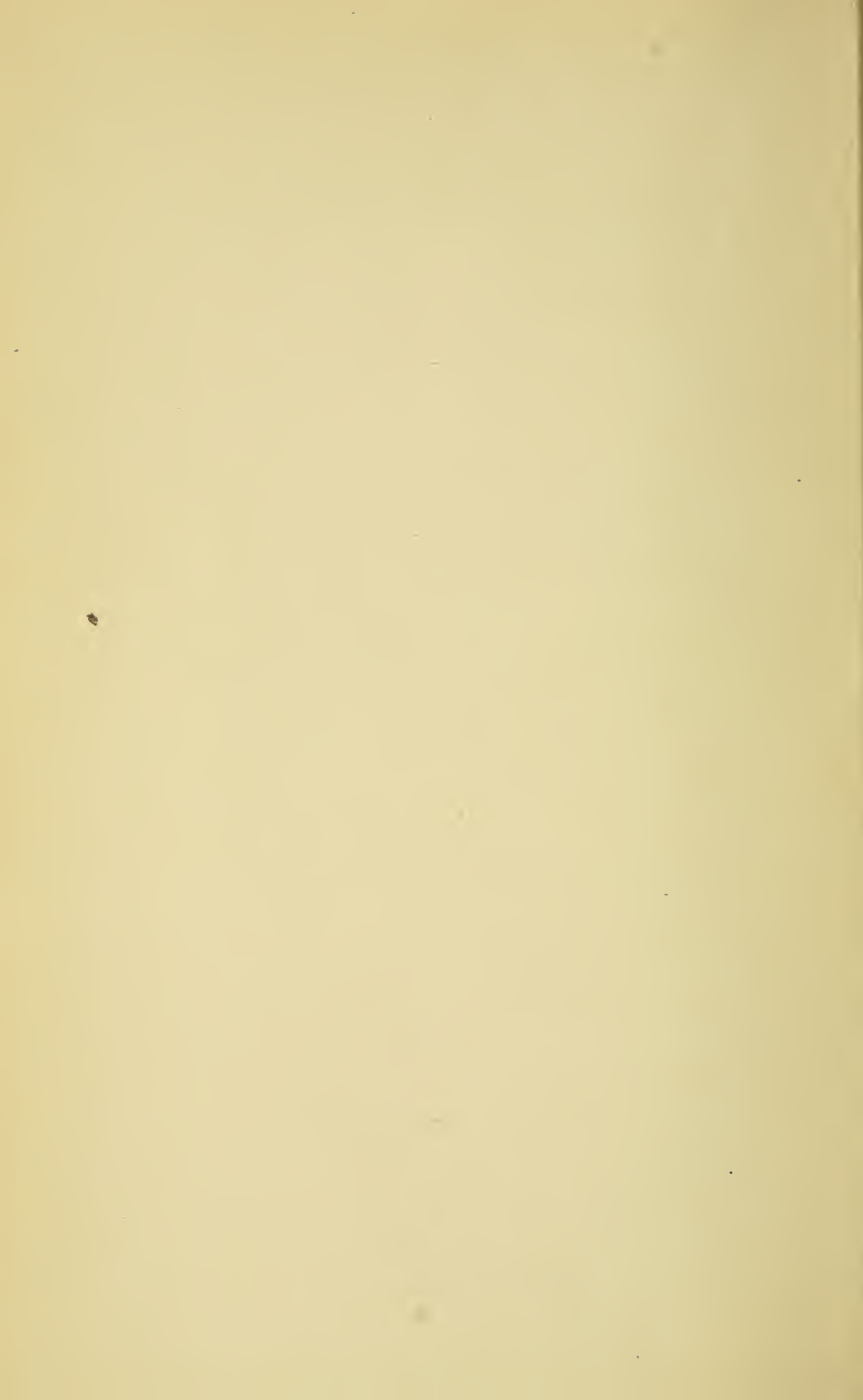
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PART I

THE RISE OF SCIENTIFIC THOUGHT





## CHAPTER I

### GENERAL

THE retrospect implied in the word "modern" may range, according to our outlook, from five years to a generation, from a generation to a century, from a century to a thousand years. In treating science historically, if we take as a test of modernity the feature which most obviously marks it at the present day, we find this in an extraordinary swiftness of rarely-swerving advance; and, when we learn what it is that has made possible the acceleration, there can be little doubt of where to define the transition from ancient to modern: it is in the seventeenth century.

Until that time, the rate of progress in what we now call science was so slow—in many times or places it was even retrograde—that a would-be student had largely to depend on knowledge handed down from perhaps two thousand years before: information gained in countries which had reached and passed an apex of natural discovery, but had bequeathed no exploratory weapon which their successors could use so as to advance from the level of thought which they had reached. In the seventeenth century, we find science almost suddenly launched on a new career, with a momentum which has hardly ever been checked since then and, however the pace may slacken in the future of one nation or another, with an impact which has altered the whole life of mankind.

So great a change could be due to no merely mechanical invention; it arose primarily and fundamentally from a spreading change in mental attitude. And when we look more closely into that crucial seventeenth century, we can perceive the focus of the matter in the founding of the Royal Society.

This event was both a token and a cause of a radical change in all forms of scientific enquiry. It is no slight to the men who were its forerunners—for their labours, although of the first magnitude, were individual—to say that this epoch marks the division between the B.C. and the A.D. of

chemical faith, began rational physiology, inaugurated Newtonian physics and astronomy, and, in general, witnessed the proclaiming and the adoption of principles by which the pursuit of all branches of Natural Philosophy throughout the world has since been ruled.

It would be as wrong, however, to infer that before 1660 no discoverer had used the true *a posteriori* method, as to suppose that thenceforward the *a priori* philosopher has ceased his operations : and these two matters may be examined a little further before the main theme is begun.

Man finds himself set in the universe, as an ant on a carpet ; and he seeks to discover the pattern from where he sits. In this unending task, doubtless every real discovery of any part of the pattern has in all times been made by the use of one and the same method in one guise or another ; but the lightning short-circuits taken by the current of thought of a genius cannot be imitated in normal minds without disaster : the methods of genius are not heritable property. Ordinary minds require the pathway of their intellectual energy to be strictly planned, and even a great brain may fruitlessly expend itself if it is not directed in the right course, or if it lacks provender to work upon. Before the rules were laid down, therefore, knowledge of nature was owed chiefly to sporadic, sometimes premature, outbursts of genius ; but thereafter it became possible for men of the highest mind to be reinforced, encouraged, and fed by innumerable others ; for these were now enabled to verify and understand their leaders' conclusions, and in turn to labour fruitfully themselves.<sup>1</sup> Given trust in their work so long as they keep the code, these enquirers have since then continuously multiplied their numbers, material, weapons, and organic strength ; until, patch by patch, a fragment of pattern is surely being perceived.

<sup>1</sup> This is perhaps as much as Francis Bacon meant to be understood when he claimed (*Nov. Org.*, I, LXI) that his method places all wits and understandings nearly on a level. Bacon's remark is an overstatement of the fact that there is now no need to depend on the *ipse dixit* of authority as the source of scientific creeds.

What will here occur to such readers as are apt, either through honest misapprehension or through prejudice, to dislike the implication of infallibility contained in the last remark, is that in the career of every science there have been theories which have proved unsound, and that there have even been periods of dogma. Few things are so dear to the middle ranks of journalism as to be able to announce the "overthrow" of the theory of this great scientist or that; and we have all suffered from the querulous complaint that, in science, what was "true" twenty years ago is disproved to-day. If this were so, then the temple of science must shut its doors, for the whole reason of its building would be gone: what is *true* is perpetually true, but there are gradations of probability. It is a prime object of any worthy form of education to enable its students to discriminate between what is sure, what is likely, and what is merely possible; this distinction is the whole task of scientific logic, and science itself aims continually at relegating men's ideas to these categories, by means of an absolute deference to external Nature. This deference is *the* essence of the scientific method; and it was this which the new philosophers of the seventeenth century preached. The practitioners of science are merely human, and so it is the case that there are some in their innumerable army—and still more among its camp-followers—who are not particularly gifted with logic, who do in fact outrun their premises, and who allow themselves to exalt mere reconnaissances of imagination into probable theory, or probable theory into permanent truth; and so they mislead laymen, besides forsaking the very canon of their own calling.<sup>1</sup> There is, no doubt, a smaller proportion of such sinners in the ranks of science than elsewhere; but, in science or out of it, their utterances do harm. Is it fair, then, to judge the scientific method by those who do not adhere to it? Or to put it aside

<sup>1</sup> In this connection it is salutary for the scientific reader (and writer) to compare Bacon, *Nov. Org.*, I, LXXXI and LXXXVI; and for any non-scientific cynic to read the second paragraph in XCVIII.



on second-hand evidence? We do not presume to judge Horace by a popular crib, Titian by photographs, Shakespear by Lamb's "Tales." The men of science may be fallible; but, within the universe of our reality, not so the method, when it is recognized as providing an asymptotic approach to truth.

It is, then, the growing use of that method which is at the heart of our present theme; and this will be illustrated by reference to a field at first wide enough, but limited thereafter within the bounds of one part of science.

No great fact of history has ever come spontaneously into full being, however suddenly it has seemed to "break surface" as chronicled action. What we are accustomed to call "the scientific method" happened to be applied to the study of "Natural phenomena" before it became the recognized weapon in the attack of other problems also; its extension to matters not always classed with Science—*e.g.*, history, textual criticism, archæology—is even yet in progress. Those who are so extending it, acknowledge their debt to the scientists who first showed the way; it is right, and profitable also, for scientists of the present day to pay similar tribute to their origins of the seventeenth century, and it is even open to them to look still further, and to acknowledge the debt which these origins owe, in their turn, to earlier movements.

To show the development and the bearing of such earlier movements is beyond the compass of this book; and the few remarks now offered merely suggest the immediate preliminaries to the period which is discussed in the next chapters.

From the middle of the seventeenth century until the opening of the twentieth, science and humanist studies were often so sharply at odds as to forget their common origin at the bifurcation of a single stem, and to obstruct the process of reunion in aim and in method which is a happy sign of the present day. The common origin in question was that quicken-



ing of the spirit of artistic inquisitiveness, which goes by the too-limited name of the Renaissance. Perhaps the Crusades are an early symptom; at all events, following a sequence which appears to have been closely paralleled in ancient Egypt, "artistic inquisitiveness" showed itself in the domain of architecture as early as the thirteenth century, and in painting two or three hundred years later; and next it touched learning, in the middle of the fifteenth century, in such a way as to cause the printing-press to be invented, for the purpose of its own furtherance. The art of printing, spreading rapidly eastwards from Germany, met in Italy the westward-moving stores of classical manuscripts brought by refugees from the fall of Constantinople; and the mating of these two engendered such a stock of classical literature and ancient knowledge as almost satisfied, for a time, the scholarly appetite of Europe.

But inquisitiveness involves independence of thought, and of action too; and so we find the spirit of the Renaissance, as it grew in strength by its own produce, invading other domains. The Reformation of the Church in Germany, the Low Countries, in Switzerland and in England grew and was accomplished; Spain awoke instead to geographical exploration, and England quickly flung itself into the same task, adding to its new-found independence and winning fresh commerce. The incentive of wealth might, conceivably, alone suffice to encourage the exploration of new knowledge, as of new countries, and without doubt this was partly the case: that Royal merchant, Sir Thomas Gresham, had in mind a very practical form of patriotism when he bequeathed the money which in 1597 gave Gresham College to London. Yet it would seem nearer the truth to think of the promotion of our commerce as only one of the ways in which Englishmen showed an awakened desire to expand frontiers of all kinds. Our merchant adventurers had settled or were exploiting new lands both east and west, in Muscovy and towards India and in the Americas; men like Gresham were founding international business by promoting the system of financial credit;

new material wealth was created, and it was ready for use in whatever changes should come to the country as a whole. But side by side with all this, abetting it and in turn fostered by it, there was the growth of emotional and purely intellectual enquiry and output. The whole English tongue, with all that it means, was being wrought into its finest shape; in the same year came the writing of *The Tempest* and the issue of the Authorized Version of Tyndale's English Bible.

## CHAPTER II

FRANCIS BACON

As long ago as the thirteenth century, Roger Bacon wrote :—

“ There are two ways of knowing : namely by statement [argumentum] and by experiment. A statement lays down, and makes us define the scope of a problem ; but it neither confirms doubt nor removes it in such a way as to give one trust in the attainment of certitude, unless it arrive at the truth by way of experiment.”

This clear statement that nature cannot be described without having been looked at was, however, not a palatable truth in Roger Bacon's world ; and not until three and a half centuries had passed could ideas of the kind gain currency. By the time of James I in England, the developments already outlined were preparing men for the suspicion that they were ignorant, perhaps even profoundly ignorant ; the way was clear for another Bacon to point out.

Born at the outset of Elizabeth's reign, the brilliant son of a high officer of the Crown and the nephew of Lord Treasurer Burghley, Francis Bacon naturally trained himself for the service of the State ; but, perhaps fortunately for what he was able to collect for later issue, he reached middle age before he was admitted to the toil of Crown offices. Under James I he rose ; from being Sir Francis Bacon he was created Baron Verulam, and in 1620, now Viscount St. Albans, he became Lord Chancellor of England. From this pinnacle he fell almost at once, a victim to official malpractices for which, fairly or not, he was impeached. During the very busiest periods of public office, and in the five years which afterwards remained to him as a private citizen, he found both time and energy to deliver his stored wisdom in the shape of the familiar *Essays* and his great works on the ‘Instauration of the Sciences.’ It is with one of the latter that we have more

particularly to deal: the *Novum Organum*, first published in 1620.<sup>1</sup>

Briefly, Lord Verulam's main thesis is that hitherto knowledge was sought by adding uncertain experience to fogged reasoning, and by adding them, moreover, in the wrong order. The new instrument which he proposes is to be an alternation of experiment with induction (which we might call "controlled guessing"); which should supplant the old *unverified* induction plus syllogism—Aristotle's instrument—in all research for knowledge. But first of all, he shows how, in interpreting nature, men's minds are continually sheering away from the facts; and he enumerates the principal infirmities which cause this and which must be guarded against if it is to be prevented. These trammels of intellect he calls "Idols"; and he names them in four classes.

There are the "Idols of the Tribe"; that is, inherited failings of the human intellect, consisting in measuring outside things by reference to human standards instead of by reference to outside things. Next come the "Idols of the Cave," which represent similar errors arising in the outlook of the individual, who allows his own private circumstances or bias to affect his observation and judgment. The "Idols of the Market-place" refer to the use of misleading language: "the ill and unfit choice of words wonderfully obstructs the understanding." The fourth group of Idols, those of the "Theatre," arises, in effect, from yielding to the authority of teachings and traditions which were themselves vitiated in the beginning by the other three idols.

Under the first heading, Bacon explains: False analogy; the neglect of contrary evidence; hasty leaping to broad conclusions—"For there is in man an ambition of the understanding, no less than of the will"; allowing feelings to colour logic:—

"For what a man had rather were true he more readily believes. Therefore he rejects difficult things

<sup>1</sup> Ellis and Spedding's translations are here used.



from impatience of study; sober things, because they narrow hope; the deeper things of nature, from superstition; the light of experience, from arrogance and pride . . .; things not commonly believed, out of deference to the opinion of the vulgar."

These, together with the coarseness of man's unaided senses in perception, make up the Idols of the Tribe.

Among the cave-idols, or personal idiosyncrasies, we are warned against making observation merely the slave of logic (*i.e.*, making observation perform only occasional service); against preferring to note differences rather than resemblances or else seeking resemblances and neglecting differences; against favouring the old because it is old, or the new simply because it is new; and against disregarding the part in observing the whole, and conversely :—

"For that school is so busied with the particles that it hardly attends to the structure; while the others are so lost in admiration of the structure that they do not penetrate to the simplicity of nature. These kinds of contemplation should therefore be alternated and taken by turns, so that the understanding may be rendered at once penetrating and comprehensive." A very modern-sounding hint.

The idols of the market-place, says Bacon, are very troublesome; for although we think that reason governs words, yet it is also true that words react on the understanding. Words, being usually framed and applied according to the capacity of the public, follow those broad distinctions which are most obvious to the understanding of the public; and whenever more acute observation or more expert understanding calls for amendment of these verbal distinctions, so as to make them accord with the real distinctions of nature, words stand in the way and resist the change. It is worth noting, as a sidelight on the height which the English tongue reached in

Bacon's day as contrasted with our own, that he says nothing of any gradual debasement in the vigour of a word's meaning, such as is common now; he dwells rather on an undue fixity of meaning. This question is, however, perhaps literary rather than solely scientific.

In all this, Bacon sets forth fully but pithily the view that while we are on the quest for natural truths, we must dismiss all our likes and dislikes, if we are to arrive at a view of things as they actually are; we must not arrogate to ourselves the power of conceiving *a priori* even a small part of the plan on which nature is devised. It would, indeed, be a cowardly matter to refuse to face the facts of the creation of which we are a part; and it would equally be presumption to imagine ourselves born with an intuitive acquaintance with its scheme of governance.

Bacon next devotes space to showing the intellectual and material advantages which a true method of enquiry would bring, inasmuch as even without it there had already been made many notable discoveries, besides useful inventions. He forestalls sundry misapprehensions concerning the position which he adopts; and, without having formally set forth his method, he enters upon its illustration in detail, by discussing (Book II) the nature of Heat. We may glean from various passages in Book I, however, a fairly clear idea of the main principles of the method.

Thus: contrasted with experience got by random gropings in the dark,

“the true method of experience first lights a candle, and then by means of the candle shows the way; beginning as it does with experience duly ordered and digested, not bungling and haphazard; and from it educing axioms, and from established axioms again new experiments.

“The induction which is to be available for the discovery and demonstration of sciences and arts, must analyse nature by proper rejections and exclusions; and



then, after a sufficient number of negatives, come to a conclusion on the affirmative instances. . . .

“ But after establishing axioms by this kind of induction, we must also examine and try whether the axiom so established be limited to those particulars alone from which it is derived, or whether it be larger and wider. And, if it is larger and wider, we must observe whether, by showing to us new particulars, it confirm that wideness and largeness, as by a collateral security. . . . And when this process shall have come into use, then at last we shall see the dawn of a solid hope.”

Thus it appears that the rungs in the ladder of investigation are :—

(1) The unbiassed mustering of a large number of facts into classes relevant and irrelevant to the matter in hand :

(2) The framing of a statement, or imagining underlying “ causes,” covering the facts in the relevant class :

(3) Examining the explanation to see what additional facts it would imply :

(4) Making new experiments to see whether these additional facts do or do not exist ; and

(5) Regarding an explanation which has passed the test of (4) as being hopeful.

It seems to us now that these steps, in that order, form the necessary procedure of anybody investigating anything, whether he call himself a scientific, a literary, a historical, or any other sort of investigator. In any enquiry, to skip over from stage 2 to stage 5, omitting stages 3 and 4—that is, to be content with an unverified explanation—is to be unwarrantably sure of the competence of the human imagination to fathom the workings of nature : and herein is expressed, I think, the essence of the scientific habit of enquiry. Some of the five stages, it is true, appeal more to one race or to one type of mind than to another ; genius, as distinct from patience

and care, may flash so rapidly through some one of them as to seem to leap altogether what others must toilsomely follow; yet, whether the five are performed by one man or by many, at one time or during years, *all* of them are necessary to establish that which natural enquiry seeks to find out. And as to the last stage, the hopefulness of an interpretation is altogether dependent upon how far it continues to withstand the assaults of the fourth process: the test of observation and experiment. Theory is saved from being dogma, not by the passage of time, but by use alone; and the longer a theory lasts, *if it is being tested*, the more rightly confident do we grow that it represents actual truth.

In the *Organum* and its associated works, Bacon does what he usually abstains from doing in the *Essays*, where human conduct is the theme; for he boldly advocates a policy: the deliberate amassing of the data for a grand enquiry into Nature, by the methods which he extols. But *Essays* and *Organum* together show, above all, Bacon's wonderful gift of dispassionately divining those human traits, which can work either for or against man in his traffic with men, but are inevitably turned against man in his traffic with inanimate nature. Here is Bacon's chief service as a teacher now. It is of little consequence for us that he could not altogether rid himself of the 'Idols' which he so clearly perceived: and in writing "There is an ambition of the understanding, no less than of the will," he was turning his brilliant mirror upon both sides of his own character. Concerning 'natural knowledge' and an all-embracing corpus of observations, we know now what he only partly saw: that there can be no quantity-surveyor for the material of science. Its depth is so great that no one age, let alone one man, can accomplish more than a fraction of the task whose rules Bacon laid down; and it is therefore unjust to insist upon his not infrequent failures in applying his own precepts. He was an architect, not a builder; or, in his own words, he "rang a bell to call other wits together."

## CHAPTER III

### THE FOUNDING OF THE ROYAL SOCIETY: 1640-1648

FOR some time, the immediate disciples of Francis Bacon were content to admire rather than to do. Yet they had noble exemplars; for already, before Bacon preached, the astronomers Copernicus (1473-1543) and Kepler (1571-1630) were practising; so, in Bacon's own day, was 'de Magnete' Gilbert (1600); so was Galileo (1564-1642); so was William Harvey; and so, afterwards, though in a different and metaphysical way, was Descartes (1596-1650). These men were great collateral offspring of the new philosophy, and were honoured as spiritual forbears by its later and more numerous children. They were the topmost peaks, they first caught the morning sun.

The first approach to a foundation which might spread Baconian principles abroad was chiefly educational in aim. It was due to the joint efforts of a naturalized British subject and a foreigner. Bacon's principles, reaching Poland, had won the admiration of the ardent educationist Komensky (Comenius), and had helped to inspire him when he framed an ambitious system of education. The gates of learning were to be entered by way of the study of language—the old material—fortified by observation—the new attack. About the year 1638 Comenius sent one of his treatises to a friend and disciple in London: a man who filled a not unimportant rôle in these times, of whom we may form some idea. His name was Samuel Hartlib; you will find a brief mention of him in *Pepys' Diary* (7 August, 1660), but he lives chiefly in his letters. Hartlib was the son of a Polish merchant of Dantzic and his English wife, and had himself married an Englishwoman. Early in Charles I's reign he had quitted his native land for London, keeping up frequent dealings with clients abroad.<sup>1</sup> He was an earnestly cheerful, bustling soul, with his finger always in somebody's button-hole, a born propagandist of new doctrines, and a valuable servant of

<sup>1</sup> *Milton, Life and Times*, by David Masson.



original minds. His connections by marriage were good; and, though he lacked the type of ability which would have raised him in that age of "climbers," he was useful on account of the warmth of his interests and the range of his friendship, both here and abroad. His charitable solicitude on behalf of more than one young man of promise, moreover, made him to be looked upon with a kindly eye by many, even when they were victims of his importunities. He gave useful service to the Commonwealth Government in propaganda-work; but later on, after the Restoration, Hartlib would seem to have got into low water, and perhaps to have returned to the Continent, where he died of the stone;<sup>1</sup> but his fate is doubtful, and it is with this defeated optimist as with poets:—

"when or where he dyed I cannot tell; for so it is, and always hath been, that most Poets dye poor, and consequently obscurely, and a hard matter it is to trace them to their graves."

Comenius was an early subject for his energies; for in 1639, Hartlib took it upon himself to translate and publish the manuscript which its author had sent him, accepting as a just perquisite any glory which may have been reflected upon himself. As soon as this English translation had done its work, the Parliament of 1641 was persuaded to invite Comenius to London. The grand educational project was claimed as "Work for a Collegiate Society," set up by the "authority of some King or Prince or Republic; in some quiet place, away from crowds, with a library and other appurtenances." Accordingly plans were developed to assign for the purpose some existing college, with its revenues; savants were to be sought from all parts of the civilized world, to take up positions at the institution. Three sites were considered: the Savoy, Winchester College, and Chelsea College; a full inventory of one of these was supplied to the Government; and now

<sup>1</sup> Cf. a letter by Evelyn, written 1703: Brit. Mus. Addit. MS. 29, p. 56.

all things were in train for carrying out the scheme of the great Verulam according to the gospel of Comenius.<sup>1</sup>

At this point it is well to recall that the tendency of the time was in favour of educational changes or innovations of almost any kind. The increasing discontent with the King's party, and growing religious divergencies, were in themselves looseners of other ties of tradition, even if we ignore the deeper agencies of which these were themselves symptoms. Little opposition to the setting-up of a new academic system was to be expected from any official reverence for the traditions of Oxford and Cambridge; for, in the eyes of the Parliament men, the Universities added to the defect of being educationally obsolete the political crime of breeding Prelacy and Royalism. This last, crime or not, was certainly true; but there are two views of the former charge, as we shall see. The only other institution which might conceivably have served the new academy either as a rival or, perhaps, as a nucleus, was Gresham College in London. This had maintained a sturdy existence since the end of Elizabeth's reign, and with its staff of seven professors it did constitute a very small, non-residential University; but, as a rival to the proposed academy, it was too small to count, whilst as a mere nucleus it would satisfy neither the pride of its trustees—the City of London and the Mercers' Company—nor the somewhat exuberant ambition of Comenius and Hartlib. Its turn was to come, nevertheless, as will appear.

Despite all these favouring conditions, the great University of London came to nothing; for Hartlib and his colleague, Parliament, and all would-be helpers, were whirled into the First Civil War. Comenius departed with all speed, went to sow educational seed in Sweden, and passes out of the affair.

For the next two years—1643 and 1644—England was a pool of by no means stagnant waters; the Revolution was in high spate, and learning was washed aside. Philosophers were forced to take sides, willy-nilly, or else to carry their work

<sup>1</sup> D. Masson, *op. cit.*



into Europe. There came at last, however, a time when London could provide quiet eddies where thoughtful men might now and then retire from the troubled waters of political and religious strife; and so, in the midst of all the turmoil, we come at length to what is for us the actual beginning of things, about the end of 1644 or early in 1645.

There now came together a small number of vigorously-minded young professional Londoners, united, in spite of the dissensions around them, by the spirit of scientific inquisitiveness; and all imbued with the New Philosophy which had been preached by Bacon and practised by Galileo. To breathe a freer air than the 'passions and madness of that dismal Age' gave forth, they formed what they named "the Philosophical College"; and this was the seed from which the Royal Society was to grow. Of the existence and doings of the club in 1645 our knowledge is owed almost entirely to the preservation of two parenthetical accounts, written years later by one of its former members, the Rev. Dr. John Wallis. They are vouched for in essentials by contemporary references in the letters of Boyle and of Hartlib; and there is therefore no reason to think that Wallis' memory was amiss, or that he here exercised the faculty, ascribed to him by an enemy, of being able "at any time to make black white and white black for his own ends."<sup>1</sup> We shall use Wallis' letter of

<sup>1</sup> The sources of this information, and the occasions of its being supplied originally, are material for judging its accuracy; they are therefore put on record here. Wallis' earlier account occurs in a letter to Lord Brouncker, P.R.S.; printed in 1678; a copy of the tract is in the British Museum (740-c-21). Here Wallis deals firmly with the aspersions of one Dr. Holder (a brother-in-law of Christopher Wren), who had accused Wallis of having stolen the credit of teaching a dumb man to speak. In one part of Holder's argument he adduced the supposed origin of the Royal Society in Oxford meetings of 1659. (It may be noted that Sprat in 1668, also Evelyn in 1703—*loc. cit.*—had a similar impression; Sprat, however, had no personal knowledge until later; Evelyn was out of England during the London period and, like Holder, he came to know the philosophers only after their migration to Oxford.) Wallis disposes to his own satisfaction of Holder's priority in the matter of the dumb; and enters into detailed

1678, interpolating in square brackets a few supplementary passages from his letter of 1697.

“ I do acknowledge, . . . that those Meetings<sup>1</sup> might be somewhat conducing to that of the Royal Society which now is: But (without disparagement to Bishop Wilkins) not that the first Ground and Foundation of the Royal Society was there laid. Which I take to be much earlier than those meetings there.

“ I take its first Ground and Foundation to have been in London, about the year 1645 (if not sooner), when the same Dr. Wilkins (then Chaplain to the Prince Elector Palatine in London), Dr. Jonathan Goddard, Dr. Ent (now Sir George Ent), Dr. Glisson, Dr. Scarbrough (now Sir Charles Scarbrough), Dr. Merrit, with myself and some others, met weekly . . . at a certain day and hour, under a certain penalty, and a weekly Contribution for the charge of experiments, with certain rules agreed upon amongst us. . . .”

It was one Theodore Haak (according to Wallis' later account) who first suggested the convening of the meetings, in which he himself took a part.

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confutation of all his allegations, including that which concerns us here; hence the account now quoted. Twenty years later, Wallis gave a very similar narrative in an autobiographical letter to a friend; this is what has usually been quoted. It comes from Birch; Birch got it from a book by Thomas Hearne the antiquarian, and the latter's publication of it is a little curious. Hearne, in 1725, published a transcription of a MS. of Robert of Brunne's fourteenth-century rhymed version of the earlier Peter Langtoft's *English Chronicle*: as editor, he laments the absence of autobiographic records of his author, urging, presumably by way of example to the shade of the long-dead Brunne, that people like Sir T. Bodley, or Dr. John Wallis (in a letter to one Dr. Smith) have had more consideration for posterity; and, while he thinks of it, as he happens to have the letter to Dr. Smith among his own papers, Hearne incontinently prints it in the Appendix to the *Chronicle*; where Birch later found it. We may be thankful for this model of valuable irrelevance.

<sup>1</sup> Referring to later meetings at Wilkins' rooms at Oxford in the sixteen-fifties

Divinity, affairs of State, and ordinary news were barred from the debates; such themes would inevitably bring in Bacon's 'Idols,' and most of these men were, moreover, surfeited with these questions of the time, which occupied them during every waking hour of the week between the end of one club-meeting and the beginning of the next. They therefore confined themselves to experimental science, or (in Wallis' words), "Philosophical enquiries and such as related thereunto; as physic, anatomy, geometry, astronomy, navigation, staticks, [magneticks, chymicks],<sup>1</sup> mechanicks, and natural experiments." Exactly what experiments were made is not recorded; but it may be presumed that Glisson and the other medical members would demonstrate simple dissections; Wilkins and Goddard were in the way of handling what might be termed physical apparatus and the latter was fond of being made their "Drudg, till they could obtain to the bottom of it"; and Wallis mentions that Goddard even kept a mechanic in his house for making lenses. We may bear in mind, that, simple and crude as the experiments and observations must have been, there was at that period no need for any other, so clear was the field before this pioneer society.

Their medical members treated of "the circulation of the blood, the valves in the veins, [the *venae lactae*, the lymphatic vessels"]. They all took a deep interest in astronomy; Goddard's telescopes and Wallis' mathematics, applied in the light of the revelations of Galileo (dead these two or three years), must have given food for lively talk; and perhaps also to some chaff of Wilkins on his hobby of a habitable moon. They dealt with

"the Copernican hypothesis, the nature of comets and new stars, the attendants on Jupiter, the oval shape of Saturn [the spots on the Sun, and its turning on its own axis], the inequalities and selenography of the Moon, the several phases of Venus and Mercury."

<sup>1</sup> 1698 letter.



The hand of Galileo's pupil Torricelli appears in their study of "the improvement of telescopes, and grinding of glasses for that purpose," with Goddard's operator to resort to; and of both of these great Florentines in the final part of the syllabus :

"the weight of the air, the possibility, or impossibility of vacuities, and Nature's abhorrence thereof, the Torricellian experiment in quicksilver, the descent of heavy bodies, and the degrees of acceleration therein; with others of like nature."

In all of which, Wallis couples Francis Bacon and Galileo as initiators of their 'New Philosophy.'

The Philosophical College is sometimes called "the Invisible College," as Boyle dubbed it, presumably from its lack of fixed quarters. Its first meetings were held either in Goddard's house or at the Mitre Tavern, near by in Wood Street; soon afterwards they had a room at the Bull Head Tavern in Cheapside. But now Gresham College enters the history; for one of the club, Samuel Foster, was professor of astronomy there, and during term-time the philosophers attended his lecture every week and then held their meeting, either in Foster's lodging at Gresham College, or at some place near; and their numbers grew during several years. Henceforth Gresham College is indissolubly a part of their background.

Some of the members of this company are especially well worth knowing. Of all of them, Dr. John Wilkins was undoubtedly a leader. His father had been a goldsmith in Oxford, where the son graduated and was ordained. At the time of our meeting him in 1645, he was chaplain to the Prince Elector Palatine, then living in London; and he had quite recently subscribed to the 'Solemn League and Covenant,' which enlisted him with the Presbyterian party. Wilkins was a strong, genial, straight-thinking parson, full of practical common-sense and tact. Long after this time, he told Pepys

that he considered "Man was certainly made for society, without which he would be a very mean creature"; and with that belief he was always to act, in the interest of unity and for the honourable settlement of discord: how successfully, may be judged from the fact that his policy of active moderation was to make him in turn Warden of Wadham, for a while Master of Trinity College, Cambridge, and (after the Restoration) Dean of Ripon, and lastly Bishop of Chester. No man in those times could hold strictly to the middle way; and that Wilkins, with his courageous tolerance, came through the country's trouble with honour from all except extremists on both sides, is sufficient testimony to him as a public man. The story of Wilkins' recommendation to his stepdaughter of the claims of Tillotson to her hand—"Betty, you shall have him, for he is the best polemical Divine in England this day"—would sadly misrepresent him, if we took it seriously; but Cromwell's niece Betty and John Wilkins between them probably had more sense of fun than most of us.

Science was a constant hobby from his student-days onwards. He had already written two books reviewing the planetary nature of the world, and the possibility of the moon being habitable; and his "Mathematical Magick," written at Oxford and to be printed before he left London, has been called the first English book on mechanics. He could not be reckoned as a direct contributor to modern science, but he was a constant nurse of its youth.

The other "Dr. J. W.," John Wallis, was at this time vicar of St. Gabriel's in Fenchurch Street, and—a more exacting post—Clerk to the Assembly of Divines at Westminster. At Emmanuel College, Cambridge, in the early sixteen-thirties he had imbibed, as he himself tells us, the principles of the New Philosophy; and Glisson, then Regius Professor of Medicine but now a London practitioner and one of his own fellows in the Philosophical club, had been the teacher to whom he especially owed this. Glisson quoted Wallis as the first of his pupils to maintain in public dis-



sertation Harvey's new discovery of the circulation of the blood. Wallis' analytical gifts first attracted notice and brought him preferment, when he exercised them in unraveling captured Royalist *communiqués* written in cipher which no one else could read. But very soon Wallis was to begin more profoundly analytical researches which made him renowned then and notable still; namely, in mathematics, so that he is rated as the greatest of Newton's English predecessors. From Wallis' *Arithmetica Infinitorum* Newton at once derived the binomial theorem; and the same work contained many elements of the differential and of the integral calculus. It is to Wallis that we owe our use of the word 'interpolation,' the use of fractional indices, and the symbol  $\infty$  for 'infinity.'<sup>1</sup> An example of his extraordinary powers in mental arithmetic is authentically recorded, namely, the extraction of the square root of a number consisting of 53 figures; this he did in the dark, correctly, with no aid but memory.

Personally, Wallis seems to have been severe and occasionally contentious; but for all this, his great learning gave him a high place in each stage of the movement which we are studying. Wallis' chief concern was with what his own brain could produce; Wilkins', with what he could help others (including Wallis) to produce.

Soon after the formation of the club, one who exceeded all of them—Robert Boyle—wrote that his chief pleasure in London was that

"the corner-stones of the *invisible* college or (as they term themselves) the *philosophical college*, do now and then honour me with their company. . . . [They are] men of so capacious and searching spirits, that school philosophy is but the lowest region of their knowledge; and yet . . . of so humble and teachable a genius, as they disdain not to be directed to the meanest, so he can but plead reason for his opinion."

<sup>1</sup> *Dic. Nat. Biog.* is the source of the foregoing mathematical points.

It was in 1646 or 1647 that Boyle joined the small society.<sup>1</sup> He had just come back to England, a delicate youth of nineteen, from the long tour in Europe on which his father, the Earl of Cork, had sent him and his brother; and he had arrived to find his father dead, his family divided, and all England at war. His elder sister, Lady Ranelagh, took him into her house in Pall Mall, where he should have both a refuge and acquaintanceship; for Lady Ranelagh was a wise and charming person, and on good terms with many of the Presbyterian folk then in power. One may perhaps imagine Hartlib, the middle-aged, benevolent tout, respectfully introducing young Boyle to the Philosophical Collegiates, in order to give the bewildered lad some occupation more congenial to his meditative mind than listening to news-laden strangers talking politics in his sister's drawing-room. At all events, the club received him well; and when he went down into Dorset to take over his manor of Stalbridge, they and he corresponded often. Hitherto, Boyle had heard little of chemistry, and it would seem that the Philosophers' talk and experiments now turned his thoughts in that direction, about the end of 1646. This was, of course, long before Boyle knew Evelyn, who need not otherwise have confessed ignorance of "who it was Initiated Mr. Boyle among the Spagiricks."<sup>2</sup>

Another young man, less brilliant than Boyle only, and a *protégé* of Hartlib, joined at about this time. William Petty was launched upon a most remarkable career, which he had begun at fifteen years of age, by leaving the clothier's shop which his father kept at Romsey in Hampshire, and betaking himself with a good stock of youthful learning and mechanical skill to study at Caen. Then he entered the Navy until he was twenty years old and (as he says) "had gotten up about 3 score pounds, with as much mathematicks, as any of my age was knowne to have had." These assets took both him and his young brother to study successively in Utrecht, Ley-

<sup>1</sup> *Life*, by Miss Flora Masson: Constable, 1914.

<sup>2</sup> Brit. Mus. Addit. MS. 4229.

den, Amsterdam, and Paris, in the last of which he was a favourite pupil of the great Hobbes, who much admired his "pregnant geny." Coming back to England with brother Anthony and a carefully-noted cash profit of £10 on his continental sojourn, he recommended himself to Hartlib and the Philosophers by an invention for making two simultaneous copies of a document; and thus began a lifelong association with them. Petty's destiny was to make him in rapid succession a physician, professor of anatomy at Oxford, professor of music at Gresham College (by which time he had "got up to £500"), physician to Cromwell's army in Ireland, Clerk to the Council and Secretary to Henry Cromwell the Lord Lieutenant, and soon surveyor of lands in all Ireland. Here his achievement of mapping the whole country in ten months was a brilliant example of his vigour in organizing and of his accurate knowledge of applied mathematics. And now he gathered substance like a snowball, for within four years or so he came out of Ireland with £13,000, fifty thousand acres of his own in Kerry, and property in London.<sup>1</sup> After the Restoration, he was knighted; and his son was the first Lord Shelburne, from whom the present house of Lansdowne derives. This recital tends to slur over the personal factors which made the career: Petty's pushful energy is evident. But he was a really learned and versatile man with a practical mind, and with a sense of humour too: he once evaded a challenge to a duel from an angry Celt by naming as weapons carpenters' axes in a small dark cellar; and his skill of mimicry sometimes embarrassed his more solemn acquaintances. As to his science, economic statisticians now look to Petty as their earliest model; and here is his permanent contribution to modern investigation. His studies of national and civic data concerning trade, population and mortality, taxes and customs, are very valuable, both for the material gathered

<sup>1</sup> The source of most of these particulars is Petty's Will, 1685, as printed in the *Annual Register* of 1761, p. 16; others are chiefly from Wood, *Athenæ Oxon.*, 1691 ed.



and for the method by which he examined it. His mode of thought as a practitioner of the New Philosophy may be seen in the following excerpt from his *Political Arithmetick* :—

[To exhibit the social and commercial condition of a country]  
“ the Method I take . . . is not yet very usual; for instead of using only comparative and superlative Words, and intellectual Arguments, I have taken the Course . . . to express myself in Terms of *Number, Weight, or Measure*; to use only Arguments of Sense, and to consider only such Causes, as have visible Foundations in Nature; leaving those that depend on the mutable Minds, Opinions, Appetites, and Passions of particular Men, to the Consideration of others.”

Here is a man to Francis Bacon's best pattern.

It is worth noting that Petty's great-grandson was the Lord Shelburne who became the patron of Joseph Priestley and the friend of Jeremy Bentham. Lord Shelburne's interest in two sciences may well be traced to a heritage from Petty; while the wealth which enabled him to support Priestley was directly due to our William's grasping habit—at which, therefore, it does not behove chemists to cavil.

These four—Wilkins, Wallis, Boyle, Petty—are a fine quartette of diverse types; and the others, though less remarkable, were important associates in what was going forward. All were devout Baconians; and hardly any were over thirty years of age, so that they had come into the world almost at the same time as *Novum Organum* and were well placed to carry its precepts into practice. Nor should it be forgotten that they owed their introduction to the New Philosophy to teaching received at the very Universities which it was the fashion in the sixteen-forties to decry. Wilkins, Merret, and Goddard were Oxford men, and Goddard had later gone for medical work to Cambridge, where Glisson, Ent, Scarbrough, Wallis, and Foster also had their training; and Haak, with

commendable Teutonic impartiality, had spent six months at each University.

The foregoing remarks do not, of course, imply that either Oxford or Cambridge was as yet a focus of experimental science; for this they had to await the return of the London philosophers whom they had formerly sent out. A note of Wallis' concerning the state of academic mathematics is worth quoting, although it refers to the slightly earlier time when he was at Cambridge in 1631.

“ At that time with us,” he says, “ Mathematicks . . . were scarce looked upon as academical studies, but rather mechanical, as the business of traders, merchants, seamen, carpenters, surveyors of lands, or the like; and perhaps some almanack-makers in London. And amongst more than 200 students (at that time) in our College” [Emmanuel] “ I do not know of any two (perhaps not any) who had more mathematicks than I (if so much) which was then but little; and but very few, in that whole University. For the study of mathematicks was at that time more cultivated in London than in the Universities.”

Another twenty years, and Wallis himself was to set this matter right for Oxford: twenty more, and at Cambridge Isaac Barrow, the first Lucasian professor, was to resign his chair to his learned “ freind, Mr. Isaac Newton, master of arts.”



## CHAPTER IV

### THE FOUNDING OF THE ROYAL SOCIETY, 1649-1665

DURING the latter part of the sixteen-forties, the London club went on with its meetings in Gresham College; but disturbances continually threatened. For the country was still fretting under violent actions and reactions: the supporters of the captured king, both here and abroad, were organizing armed forces and stirring up risings to restore him; the anti-Royalists in power were quarrelling among themselves; the fleet was disaffected; Scotland was in a dangerous temper; over the whole land there were mustering, marchings, little riots and one-day risings, scares, flights, and rumours; and Cromwell, Ireton, and Fairfax, with their army at their backs or tugging them on, were all driving towards the final desperate ending of the monarchy. With London now in the vortex, it is little wonder if the quiet meetings of the virtuosos began to be interrupted or deferred; and that they made, as they did, a triumphant recovery and advancement was owing to the state of affairs, not in London, but in Oxford.

Oxford had surrendered to Parliament in 1646; and with the ending of the First Civil War, the acting Government determined to do at Oxford what they had already done at Cambridge, namely to rid it of partisans of Prelacy. We have a lively description, from that vinegar annalist Anthony à Wood, of the first results of this news.

*"Anno 1647.* This year flocked to the University several poor Scholars, whom some called the scum of Cambridge, many poor Schoolmasters, Pedagogues from Belfries, Curates and sometimes Vicars, as also Parliamentary soldiers, especially such as had been lately disbanded, to gain preferment by the Visitation approaching. . . . They were commonly called *Seekers*, were great frequenters of the Sermons . . . preached by the six ministers appointed by Parliament, and other Presbyterian

ministers . . . and sometimes frequenters of the Conventicles of Independents and Anabaptists. The generality of them had mortified Countenances, puling voices, and eyes generally when on discourse lifted up, with hands laying on their breasts. They mostly had short hair, which at this time was commonly called the Committee Cut, and went *in quirpo* <sup>1</sup> in a shabbed condition, and looked rather like Prentices, or antiquated School-boys, than Academians or Ministers. . . . After the entry of the said Parliamenters, there appeared nothing but confusion, darkness, &c. Hell was broke loose upon the poor remnant, and they were overrun by Sectaries, Blasphemers, Hypocrites, Exciters to Rebellion, Censurers, Covetous Persons, men of self-pride, envy, and what not. So that those of the Gown that could not brook such persons, did either leave the University, or abscond in their respective houses, till they could know their doom by the approaching Visitation."

The Visitation came; and it was pretty rigorous, though more tolerant in some quarters than had been expected. Luckily for later biographers, Anthony Wood himself remained, through influence, "otherwise," he says, "I had infallibly gon to the pot." Though the swarm of parasitic Roundheads was dispersed, many a good Loyalist was ejected from his post; but when the Government eventually put in its men, it called upon the leaders of the London virtuosos, one after another. Wilkins went first, in 1648, to become Warden of Wadham. Wallis followed next year as Savilian Professor of Geometry. Petty went at about the same time, first to assist and soon to succeed a professor of anatomy who could not bear the sight of blood. When Goddard returned from campaigning in the North, Oliver, who had rated his company and counsel highly, had him made Warden of Merton in 1651.

<sup>1</sup> Uncloaked.

All that we hear of the London club for a number of years now is that it still existed, though depleted of its stalwarts, and with Boyle generally out of town; and that it served as a centre for any of its founders who visited London from Oxford. This probably means that a few habitués still 'dropped in' at Gresham College for the lecture in astronomy or geometry; but the strength had gone from the club for the time being, and the centre of gravity was now at Oxford.

Wilkins lost no time in organizing a new club. Besides the other Londoners, he had Seth Ward, the new Savilian Professor of Astronomy (afterwards Bishop of Salisbury), and Laurence Rooke, another astronomer and experimentalist; both of these had come to Wadham so as to be near Wilkins. Then there was Thomas Willis, a Royalist physician and a notable physiologist, to be mentioned again later; his wife was the sister of that Dr. Fell whose name is so humanly remembered in the rhyme. The meetings were held in Petty's rooms, for the sake of the chemicals and other material to be found in the apothecary's shop below. It is evident, from the set of rules dated 23 October, 1651, which are the only surviving record of this society, that it was at this time regularly constituted. Its members were elected by ballot; the society owned funds and apparatus; it held meetings weekly; and on each occasion one of the members was appointed to arrange the day's experiment, under a forfeit of half-a-crown. All this recalls Wallis' account of the parent club in London.

Before long Petty went off to seek his fortune in Ireland; and we now find the company meeting in Wilkins' rooms in Wadham, where he had gathered together an interesting collection of instruments and curios. And now the cult of science in Oxford grew prominent, and we meet the names of a number of young men who esteemed themselves virtuosos.

Parenthetically, it would seem that the experimenters who popularized science were now responsible also for popularizing coffee; and some of the "old brigade" regarded them



in both respects as detrimental. "Why doth solid and serious learning decline? . . . Answer, because of coffea-houses." Poor Anthony Wood! The way of the conservative historian is hard; for him it is "never jam to-day."

Wilkins now drew in a trio which is probably without match: Robert Boyle, Christopher Wren, and Robert Hooke. Boyle came in 1654 to Oxford as the only place where he could work unhindered, and among the congenial spirits by whom he had first been urged to the New Philosophy. Wren had been for some five years at Wadham, had just graduated M.A., and was newly a Fellow of All Souls. Early in his course he had distinguished himself in mathematics—he had episcopal forbears, and bishops in those times seem to have had a mathematical knack—and his later services in this field were such, that Newton bracketed him with Wallis and Huyghens as *facile principes* among the geometers of the age. Just now, however, at twenty-two years of age, he was making a name in physiology, for he and Dr. Willis were the first to inject fluids into the veins of animals; and at the same time he was amassing a store of astronomy and of the applied mathematics which were to become essential in his later and better-known career. Before long he was to hold the chair of astronomy at Gresham College; and later still, in 1661, he succeeded Seth Ward in the Savilian chair of astronomy at Oxford. It was not until the 'sixties that architecture claimed him; but even then he kept up scientific work. From 1680/1 to 1682 he was President of the Royal Society. All his life he was full of fruitful suggestions in all branches of science, and himself made many advances, which were usually incorporated in the work of his friends. Thus, he suggested to Boyle the making of observations on barometric pressure, which were the basis of our first systematic weather-reports; and Wren invented what seems to have been the first clock-work recording-drum, which he used to make a weathercock self-registering, and afterwards applied to thermometers; and he devised an integrating rain-gauge. It was Wren's method



of sprinkling iron filings over a magnet on a card, which Faraday was to use again one and a half centuries afterwards, for discovering lines of force. He was an authority upon the simple and compound pendulums, and upon a number of questions in mechanics, in astronomy, and in the theory of navigation. With all his depth of learning and doubtless because of its width, Wren was full of modesty. His wonderful compass in scientific and artistic creation is probably paralleled only by Leonardo da Vinci's.

Robert Hooke was Christopher Wren *minus* the suavity of that graceful genius, and with a restless scientific fervour in the place of Wren's great mode of emotional outlet. Hooke was the weakly son of a poor parson in the Isle of Wight, and had a boyhood like Petty's, acquiring all sorts of arts as well as grammar; he was apprenticed to Peter Lely the painter, but left him when the smell of paint made him ill, and went to school under Dr. Busby at Westminster. Thence he came to Oxford as a Christchurch chorister; and in the early 'fifties Dr. Willis engaged him as his chemical laboratory assistant. When Boyle arrived in 1654, Hooke became his assistant instead, and was brought more and more into the company of the virtuosos. He was a hollow-cheeked, sallow, wry-bodied little man; a "fretful porpentine," far too ready to stick a quill into anyone whom he suspected of impinging on his own discoveries; very jealous for his own credit, in that uncomfortably assertive way sometimes seen in an unprepossessing person with a good brain. But Hooke's brain was not merely good, it was that of a genius; it has been said that Newton's advent was the only reason that Hooke did not overtop all others of his period. To begin with, his skill in devising physical instruments and in constructing them enabled him to make observations more numerous and more exact than those of any of his contemporaries; and he had the quantitative mind, united with a fertile imagination which was well controlled by the common-sense principles learnt from Boyle and his early associates. He ranged over so

many problems that in any one of them he would sometimes go only just far enough to convince himself, not far enough to convince others; and so he arrived at inklings of many more truths than he proved. Nevertheless, the mere list of his doings and inventions suffices to exhibit him as a real Olympian. His *Micrographia* (1665) is a magazine of very various observations, from surface-tension to gnats' eyes, bookworms to combustion: 'Hooke's Law' is found now in any text-book of physics; Hooke's Joint is well known in engineering; he was one of the first who began to understand combustion in air; we owe to him (and to Huyghens, independently) our watches driven by spiral springs, not to mention numbers of present-day instruments of precision; and it is hard to say how far the success of Boyle's air-pump was due to Hooke's ingenuity. When he came, later on, to London, Hooke's experimental energy made him the pivot of the Royal Society; and later still, with all his work here and in the geometry chair at Gresham College, he followed Wren into architecture, helping him with St. Paul's, and himself designing the second Bethlehem Hospital, Aske's Hospital in Hoxton, and the first Montague House on the site where is now the British Museum.

With such men, then, at Oxford in the later sixteen-fifties: with Boyle, Hooke, Wallis, Wren, Willis, Ward, Rooke, all in one coterie, and Wilkins ringing the bell to call these wits together, a great school was founded. Indeed, it would seem that they no longer felt a need for the stimulus of formally arranged meetings; Hooke tells us that much work was done, but that they kept no minutes of the discussions which took place from about 1655; their labours then and later were to be guessed only from books published by individual authors years afterwards. Early in 1657 Petty writes expressing his pleasure at hearing that "the club is revived at Oxford"; but it must have been only for a brief space upon a formal basis, if at all: a new factor arose in 1657; and Wallis, referring to some period prior to 1659, says: —

“Not but that ingenious persons in Oxford, as they met occasionally, (whether in those [Wadham] Lodgings, or elsewhere) did oft discourse of Philosophical affairs; But the *Set Meetings* for such purpose (which had before been there) were then disused, and had been for a good while. And, what was of this nature at Oxford (about Experimental Philosophy) in those days, was rather at Mr. Boyle’s Lodgings, than at Wadham-Colledge.”

Thus we pass to the next stage; for in the last years of the Protectorate London and Gresham College again attracted the scientists. They began to come up to attend Rooke’s lecture in geometry on Thursdays, and the newly-appointed Wren’s lecture in astronomy on Wednesdays at 2 o’clock; and thereafter they used to assemble in Rooke’s rooms in the College, just as many of them had done a dozen years before, in Foster’s day. Then, in 1660, came the Restoration of the Monarchy. A fair and settled future lay before all moderate men, of whom our friends certainly hoped to be reckoned, indeed, the infusion among them of several virtuosos nearly connected with the Court, made them appear not merely moderate, but zealously royal. At all events, here was at length the opportunity for that which was in all their minds.

One Wednesday, 28 November, in 1660, a dozen habitual attendants gathered at Gresham College, and someone raised the idea of founding a College. It was an idea which in one form or another had floated in the thoughts of more than one of them since the days of Comenius and Hartlib, but the civil troubles, combined with the Oxford migration, had hitherto prevented it from maturing. There were gathered now Wilkins and Goddard, of the very first London philosophical club of 1645; Boyle and Petty, who had come into it soon after them; Laurence Rooke, who had been in the Oxford club pretty early, if not from the first; Christopher Wren,



whose lecture they had just heard; Lord Brouncker, Sir Robert Moray, and Sir Paul Neile, cultivated men who had various connections with the new Court; Mr. Bruce, Mr. Ball, and Mr. Hill, of the Temple. After discussion, they saw that they need meanwhile look no further than themselves for their College; and decided to found a society there and then. Dr. Wilkins was naturally chosen as Chairman; Mr. Ball, the lawyer, as Treasurer; and as "Register" or Secretary, Dr. Henry Croone, the professor of rhetoric at Gresham College.<sup>1</sup> An entry-fee of 10s. and a weekly subscription of 1s. were fixed; provisional rules were drawn up; and a list was made of forty-two men who should be invited to join. Among these may be noted Wallis, Ent, Glisson, Merret, Scarbrough, and Willis, whom we have already met; Elias Ashmole; John Evelyn; Abraham Cowley the poet; Mr. Povey and Mr. Pett, out of *Pepys' Diary*; and Henry Oldenburg, the late agent for Bremen, a man and a scribe, much on the pattern of old Hartlib, and well known to Boyle and others at Oxford for four or five years past.

On the following Wednesday Sir Robert Moray brought the King's verbal approval of the foundation; and with the order that Mr. Wren prepare against the next meeting for the Pendulum Experiment, the Society inaugurated its concerted work.<sup>2</sup> A little later, the courtier Scot, Moray, was made President, in whose honour the foundation-day has ever since been commemorated on St. Andrew's Day (30 November). On 15 July, 1662, the charter was sealed which formally incorporated "The Royal Society for the Improvement of Natural Knowledge." Lord Brouncker, a mathematician of

<sup>1</sup> Croone was kin somehow to Boyle, and his father had owned the King's Head Tavern, one of Ben Jonson's haunts. His memory was afterwards perpetuated by his widow, who founded the Croonian Lecture which is annually given at the Royal Society.

<sup>2</sup> Wren has thus another distinction which is worth remembering of him.



some parts, was the first President of the Royal Society; Oldenburg was Secretary, with Wilkins as a nominal colleague. The motto chosen, one of several submitted by John Evelyn, is 'Nullius in Verba.'<sup>1</sup>

It should be understood that the Royal Society was not (nor is it now) solely a gathering whereat papers were read, for subsequent publication. It was the home of a central laboratory and museum, and it maintained a curator of experiments as a salaried officer (Robert Hooke, from 1663); its Fellows, in prosecuting their researches either at Gresham College or in their own private laboratories, were guided very materially by the discussions which arose and the decisions which were reached upon their reporting or demonstrating their results to the Society. Very often the Curator, or a specially-qualified Fellow, was required to carry out and report upon the necessary experiments. All such reports, and the directions of the Society as to the further course of the researches in hand, were recorded by the Secretary in the books; but no official journal was issued for the first ninety years of the Society's existence.

The *Philosophical Transactions*, which began to appear in 1665, soon after the incorporation, were, until 1750, officially a private venture of the Secretary, published at his expense and sold to Fellows and to the public for his profit; but the Council kept a watchful eye upon them, and was sometimes prepared to indemnify the Secretary against monetary loss. Frequently a paper which had been given to the Society would be described in *Phil. Trans.*; full publication of the researches was left to books by their authors, sometimes with the imprimatur of the President. We owe to this practice one conjunction which is especially pleasing, especially English, and no doubt the despair of pure-minded persons:—

<sup>1</sup> From Horace, *Epistles*, I, 1, line 14:—

Nullius addictus iurare in verba magistri—

Not under bond to abide by any master's authority.

PHILOSOPHIÆ

NATURALIS

PRINCIPIA

MATHEMATICA.

Autore JS. NEWTON, *Trin. Coll. Cantab. Soc. Matheseos*

Professore *Lucasiano*, & Societatis Regalis Sodali.

---

IMPRIMATUR.

S. PEPYS, *Reg. Soc. PRÆSES.*

*Julii 5, 1686.*

The *Phil. Trans.* contained scientific news from all available sources, together with book-reviews and articles, and fulfilled much the same function in the seventeenth century as *Nature* does in the twentieth.

The Society appointed committees to collate information on numerous branches of science, such as anatomical, chemical, "georgicall," industrial; and to organize scientific enquiries in remote countries. These committees seem to have been not idle, for example that on mechanical inventions was instructed to meet at Lord Brouncker's house on the first and third Saturdays of the month at 9 in the morning. A plaintive private scribble by the Secretary of the early 'sixties shows well the scientific propaganda which were an invaluable function of the Society <sup>1</sup>:—

" The business of the  
Secretary of the R. Soc.

" He attends constantly the meetings both of the Society and Councill; noteth the observables, said and done there; digesteth thm in private; takes care to have thm entred in the Journal- and Register-books; reads over and corrects all entrys; sollicites the performances of tasks recommended and undertaken; writes all letters [a]broad

<sup>1</sup> Brit. Mus. Addit. MS. 4441.

and answers the returns made to thm, entertaining a corresp. wth at least 30 psons, employes a great deal of time, and takes much pains in satisfying forran demands about philosophicall matters, disperseth farr and neare store of directions and inquiries for the Society's purpose, and sees them well recommended etc.

“ Qy. Whether such a person ought to be left unassisted? ”

We may recall that the investigations undertaken by the Society, though largely in pure science, were not confined to it, and extended also to the practical arts; and even improbable assertions were given the chance of proving true by being put to the test of experiment. In illustration of these points, a few extracts from the Journal Book of 1660–1661 may be quoted.<sup>1</sup>

“ Lord Brouncker was desired to prosecute the experiments of the Recoyling of Gunns, and Mr. Boyle, his Cylinder. [Engine for the Exsuction of Aire.”]

“ Dr. Petty was intreated to enquire in Ireland for the petrification of wood, the barnacles, the variation of the compass, and the ebbing and flowing of a brook.”

“ Mr. Povey was intreated to send to Bantam for that poyson, related to be so quick as to turne a man's blood suddenly to gelly.”

“ A circle was made with powder of unicorn's horn, and a spider set in the middle or it, but immediately ran out severall times repeated. The spider oncc made some stay upon the powder.”

“ The experiment of the compression of water was directed to be tried by Dr. Wilkins and Dr. Petty.”

“ Experiments were made at the Tower of London on the weight of bodies increased in the fire.”

“ Dr. Clarke was intreated to bring in the experiment of injection into the veins.”

<sup>1</sup> An interesting collection of these, taken from Birch's *History of the Royal Society* by Mr. T. E. James, is printed in *Nature* of 1924, passim.



“ Sir Kenelme Digby related that the calcined powder of toades reverberated, applied in bagges upon the stomach of a pestiferate body, cures it by severall applications.”<sup>1</sup>

“ Mr. Boyle brought in his account, in writing, of the experiment hee made of the compression of aire with quicksilver in a crooked glasse tube.”

“ Dr. Goddard was desired to give an account of his dissection of the chameleon.”

“ That the Society write to Mr. Wren, and charge him . . . to make a globe of the moon, and likewise to continue the description of severall insects.”

“ A report was made of the trial of the dyving engine at Deptford on Friday precedings by the amanuensis, who stayed in it eight and twenty minutes under water.”

“ Mr. Henshaw read his History of the Making of Saltpetre.”

“ Mr. Boyle to try the velocity of sounds.”

In all the tests, measurements, and reports which were brought forth, many of them fundamental, the guiding principle was the same. Croone puts it in a letter written to Dr. Power of Halifax in 1661:—

“ . . . you may please to understand that this Company do's not take upon it selfe to assert any one Hypothesis but every man is left at present to his Freedom; for they believe that to make any Hypothesis, and publickly owne it, must bee after the triall of so many experiments as cannot be made but in a long tract of time.”

And in the draft of a minute, written probably by Hooke, we find:—

“ —this Society will not own any Hypothesis, Systeme, or doctrine of the principles of Naturall Philosophy, proposed, or maintained by any philosopher Auncient or

<sup>1</sup> “ But the truth is, Sir Kenelm was an errant mountebank.” Evelyn's *Diary*, 7 November, 1651.



Moderne. . . . Nor dogmatically define, nor fix Axioms of Scientificall Things, but will question and canvas all opinions, adopting nor adhering to none, till by mature debate and clear arguments, cheifly such as are deduced from legittimate experiments, the trueth of such positions . . . be demonstrated invincibly."

No better statement could be made of the basis upon which real science has ever since rested.

It is worth while to know how the Society's doings appeared to outsiders. Charles II, the titular founder, maintained his customary attitude of jesting indifference—"Nothing but weighing of air"—on the rare occasions when he thought about it at all. There were not wanting philosophers of the older school, such as Stubbe of Warwick and Hobbes of Malmesbury, who published violent attacks on these anti-Aristotelian upstarts, or confuted their science "with a Pish, or a great word or two"; but such attacks were stoutly met by lay supporters of the Society, in particular the Rev. Dr. Sprat and the Rev. Joseph Glanvill, rector of Bath. Sprat's well-known *History of the Royal Society*, which is less a history than an essay and defence with illustrative documents, was published in 1668 after repeated proddings by Wilkins; but for all its author's dilatoriness, it is a fine and mellifluous work. Glanvill is a lively and hard-hitting writer, who did more than write, for he founded one of the provincial outposts of the Society in Somerset. There is preserved <sup>1</sup> a long set of verses by him which give an idea of the notion formed about the Society by the 'man in the street' of 1663; some stanzas may be quoted, if only for their appalling torture of poetic principles:—

Our Marchants on th' Exchange doe plott  
To encrease the kingdoms wealth by trade;  
At Gresham Colledge a Learned Knott  
Unparallel'd designes have layd  
To make themselves a Corporation  
And know all things by Demonstration.

---

<sup>1</sup> Sloane MS. 1326.

To th' Danish agent late was showne  
That where no aire is ther's no breath  
A glass this secret did make knowne  
Wherein a catt was put to death :  
Out of the glass th' aire being screw'd  
Pusse dy'd, and neere so much as mew'd.

These men take nothing upon trust,  
Therefore in councell sate many Howers  
About fileing Iron into dust  
T' experiment the Loadstones Powers;  
In a circle on a board they strew it  
To see by what lines the Loadstones drew it.

This learned [company] consists  
Of men of Honour and of parts :  
They're Lords and Knights, Physicians, Priests,  
All skilled in sciences and arts.  
Solomons in Nature, and can read her  
Ev'n from ye Hyssop to ye Cedur.

We may possibly guess, after reading these verses of Glanvill's, for what purpose the outraged poets of the Society commanded Mr. Povey to get the quick poyson to turn a man's blood suddenly to gelly.

To the many foreign visitors, the meetings seem to have served as enviable examples of quiet, orderly conduct, free from either excited arguing or pomposity.<sup>1</sup>

Finally, let us take one other view of them—Pepys'.

“ To Gresham College, where I had been by Mr. Povey the last week proposed to be a member ; and was this day admitted, by signing a book and by being taken by the hand of the President, my Lord Brouncker, and some words of admittance said to me. But it is a most acceptable thing to hear their discourse, and see their experiments, which were this day on fire, and how it goes out in a place where the ayre is not free, and sooner out where the ayre is exhausted, which they showed by an engine on purpose. And this being done, they to the Crowne Tavern, behind the

<sup>1</sup> Cf. a passage by Sorbière, 1663, which is quoted by Weld in his *History of the Royal Society*, I, 167–8.

'Change, and there my Lord and most of the company to a Club supper; Sir P. Neile, Sir R. Moray, Dr. Clarke, Dr. Whistler, Dr. Goddard, and others, of the most eminent worth. Above all, Mr. Boyle was at the meeting, and above him Mr. Hooke, who is the most, and promises the least, of any man in the world that ever I saw. Here excellent discourse till ten at night, and then home."

And there we may leave our pioneer philosophers, in the sleep which they have surely earned. For, despite ups and downs, the Society flourished, and has from that day onwards been the focal point of science in England, and indeed, in the world. Its methods and its teaching spread from London back to Oxford and to Cambridge, to Ireland, to Scotland, to New England,<sup>1</sup> to France, Holland, Germany; as its enthusiastic secretary Oldenburg said, it fermented all Europe; and it was the prototype of all British and of many foreign scientific societies. Not suddenly and grandiloquently begun, but in English fashion slowly grown and grafted on to a small, long-established institution, the Royal Society was at last the practical realization of the forty-year-old teaching of Francis Bacon Lord Verulam.

#### APPENDIX TO CHAPTER IV

##### *Missionary Science in Britain and America*

Some notes on the daughter-colonies of the Royal Society may be of interest. Of the OXFORD Philosophical Society a brief mention has already been made: it began in 1683, under the tutelage of Wallis and Dr. Plot, and it lasted for about eight years. Its members were chiefly of a generation of students or young dons who caught the after-glow of the pioneer philosophers whom we have followed; and although they might not claim any very memorable discoveries, they pursued their enquiries with great verve, and served well the purposes of the

<sup>1</sup> See Appendix to this chapter.



senior body.<sup>1</sup> (It may, in this connexion, be stated that the note in Weld's *History of the Royal Society*, I, 35, respecting a supposed arrangement for conducting certain Royal Society trials by the Oxford Society, is based on a misapprehension; for the letter there cited by Weld as being from John Wallis is in fact signed "J. W.," and a comparison of its contents with Evelyn's *Diary* for 4 August, 1665, indicates that it was written by John Wilkins, and relates to certain trials which he and Hooke and Petty were conducting at Durdans while taking refuge from the Plague.)

At CAMBRIDGE, less success was met with, despite the efforts of Isaac Newton and others. On 23 February, 1684/5, Newton writes that their efforts to found a society have failed, and "that which chiefly dasht the business" was the lack of anyone in Cambridge willing to try experiments. They have made up for it since!

In IRELAND, a philosophical society in Dublin was begun in 1683, with the help of Petty; a melancholy correspondent writes:—

"If the Dublin Society for the improvement of real knowledge could but follow (though at a mannerly distance) as they endeavour to imitate the Royal Society of London, I would then hope to breathe, even in Ireland, a better air."

This society, like its twin in Oxford, lasted for some years before ceasing; but about 1705 it was revived with considerable prestige, and is the original of the Royal Dublin Society.

In SCOTLAND, an early sign is due to the Oxford Philosophical Society, which sent out presidential letters in 1684 to the heads of Scots universities, inviting scientific correspondence. The Oxford minutes record a reply of 17 January, 1684/5, from St. Andrews, in which Mr. Cunningham of St. Leonard's College expresses his great readiness to help Scots

<sup>1</sup> See Mr. R. T. Gunther's edition of their minutes (*Early Science at Oxford*, Vol. IV; Oxford, 1925).



propaganda, and encloses a letter from Dr. Skene, Provost of St. Salvator's College in St. Andrews, concerning the establishment of scientific correspondence between the Oxford Society and Dr. Skene with his friends. The Society thereupon ordered a copy of its minutes to be made, for transmission to Dr. Skene; it would be interesting to know if this has survived. The *Phil. Trans.* of this period has at least one contribution from Aberdeen; it may be a fruit of the Oxford correspondence.

Edinburgh University began a weekly "virtuoso meeting" in 1705, as is recorded by Weld (*History of the Royal Society*, I, 383); apparently this was the seed which had fruition in the Royal Society of Edinburgh seventy-eight years afterwards.

The AMERICAN connexion with the Royal Society has lately attracted attention owing to the Society's sale of a book which it had received in 1667 from John Winthrop, of New England. There are several references to the stimulation of American academic science in Boyle's correspondence, whence some excerpts may be given. The first is in a letter from Dr. J. Beale of Yeovil in Devon, and is dated 31 October, 1666:—

"SIR,

"I have moved Mr. Oldenburg to find a way to use Sir Robert Atkins the recorder of Bristol, and what means else he could to frequent the correspondence with our American plantations, and more particularly with New England, amongst whom there is some fear of God, but too notional in the bulk of wavering creeds. They may grow as we shrink. They are the granary, and husbandmen for all other colonies. They begin for a university, and by the paucity of seducing libraries, and the necessity of useful and accomodable supplies, may be at leisure to be imbued with our suggestions. And it is fit to put new wine into new vessels."

The next is from Oldenburg (Sec. R.S.), dated 15 October, 1667. He says he has been sending off copies of Sprat's *History*

*of the Royal Society* (then just published) to Florence, Madrid, Paris, and Dantzic; and

“ I cannot but send also one to New-England, for Mr. Winthrop, having now a very good opportunity for doing so; and, at the same time, in my letter, solicit him to endeavour to season the youth of New-England with our experimental philosophy.”

Is this volume of Sprat, with or without Oldenburg's letter to Winthrop, at Harvard?

Five years later, is a letter from Leonard Hoar, a new arrival in Cambridge, New England; it is dated 13 December, 1672, and parts which concern Harvard are worth quoting here. It may be recalled that Boyle was the head of an organization for supplying the New Englanders with bibles and was a heavy subscriber of money for this. Hoar opens with expressions of their general gratitude on this score, and proceeds to mention numerous botanical and other specimens which he is sending Home. His botany teacher, Mr. Alexander Balaam, is, he says, at present on a visit to England.

“ It hath pleased even all to assign the college for my Sparta. I desire I may adorn it; and thereby encourage the country in its utmost throws for its resuscitation from its ruins. And we still hope some helpers from our native land. . . .”

“ A large well-sheltered garden and orchard for students addicted to planting; an ergasterium for mechanical fancies; and a laboratory chemical for those philosophers, that by their senses would culture their understandings, are in our design, for the students to spend their times of recreation in them; for readings on notions only are but husky provender.”

Verily, a New Atlantis! That design, if it was carried into effect, would give probably the earliest specially-built university chemical laboratory in the world.

“ And, Sir, if you will please of your mature judgment and great experience to design us any other advice or device, by which we may become not only nominal, but real scholars, it shall, I hope, be as precious seed, of which, you and me and many by us shall have uberous provent at the great day of reckoning.” A far-seeing colonist.

There are other letters of interest, too long to quote : for instance, from William Avery of Boston, New England, and his son, both assiduous chemical experimenters (1682); from John Winthrop, at Boston (1670), and some (1664-1673) of considerable interest in the history of Colonial politics and religious liberty, from the Governor and General Court of New England, addressed to Boyle as the special curator of their liberty and interest with the Throne.

## PART 2

# THE GENESIS OF MODERN CHEMISTRY





## CHAPTER V

### “ THE FATHER OF CHEMISTRY ”

HAVING now seen something of the establishment of experimental science in general, we shall examine some applications of its principles to chemistry in particular. The turning point of our science is marked by the publication in 1661 of that extraordinary work *The Sceptical Chymist*, written by Robert Boyle. It contains the fruits of the labours which he began at Stalbridge and developed at Oxford; and it might fitly be called the Book of Genesis of Chemistry.

In Boyle's time, chemistry was chiefly in the hands of three kinds of men : first, those who used its operations in order to prepare remedies and nostrums—the pharmacologists of that day; second, those who sought to create gold—the alchymists proper; third, the schoolmen, who sought (or claimed to possess) a theory of matter. Naturally, the drug-makers, the gold-makers, and the theory-makers were not sharply distinct, and a single practitioner could unite all three interests.

It would be more natural for us, at a distance, to exaggerate the services of the theory-makers, than it would be for Boyle, as a reformer at close quarters; and we should therefore bear in mind that the very bulk of the practical knowledge which had become available during many centuries from Egyptian, Arabic, and European sources, obscures the fact that it had degenerated into rules-of-thumb.

As to applied chemical *science* in a real sense, there was none, for as yet there was no sound chemical theory founded in reality, nor even any good classification, to apply. The soap-boiler, the ore-smelter in Joachimsthal or Cumberland, the gunpowder-maker at Waltham Abbey, derived neither help nor explanation in their processes from men who either were simply “ sooty empirics ” on a scale smaller than their own, or else too often were thinkers wrapped in mystery, who failed to seek in the detailed behaviour of matter any clue to the nature of matter. This is not to say that craftsmen dealing with chemicals did

not pursue their arts by way of experiments; but this does not make them scientists, for the *prime* object of science is to find explanations, not to produce effects.

In the middle of the seventeenth century, the medicinal operators ('iatrochemists') were the most prominent: and it is difficult now to realize what a widespread lay interest was taken in their remedies. In ordinary letters of that day from one friend to another, one meets recommendations of supposed medicines for all manner of ailments, the symptoms of which are usually retailed with a revealing frankness and gusto, that might even nowadays raise eyebrows. Again: the learned and pious Mersenne, in the middle of a chapter of a mathematical work, describes a cure for erysipelas, and proceeds with a medicine good against corns. Sir Kenelme Digby, an original F.R.S., whose learning and culture were as famous as his romantic and his martial exploits, fed his wife upon capons fattened with the flesh of vipers, this being thought a powerful recipe for the preservation of health and beauty; and when, soon afterwards, she died, Digby retired into seclusion in Gresham College, with all the marks of mourning, to pursue with undiminished confidence his studies in medicinal chemistry.

Boyle himself took the deepest practical interest in such matters, and many of his preparations, such as his "edulcorated colcothar carried up with sal armoniac" (a preparation containing iron), were eagerly sought after by his acquaintance. "Take," says Boyle, in beginning the prescription for another compost, "Take of the blood of a young man as much as you please"; again, "Salt of man's skull, so much extolled against the falling sickness": here we see hints of the half-magical cures still much in vogue, and definitely exemplified in numberless other cases. A skin trouble, according to one physician, could be healed by pounding some of the affected skin with oak-leaves and pegging the mixture into a hole on the eastward side of an oak tree: and it is related, of one blessed with the name Arise Evans, that he

"had a fungous nose, and said it was revealed to him

that the King's hand should cure him; and at the first coming of King Charles II in St. James's Park, he kissed the King's hand, and with it rubbed his nose; which disturbed the King, but cured him."

Two or three years ago, a woman visited the Morgue in Paris, with the object of getting a part—I think it was the hand—of one of the bodies, so as to heal an ailment by applying the hand to the part of the patient affected. This is a long survival of an old and gruesome superstition, in which, three hundred years ago, even the great William Harvey believed. In his case, he 'cured' tumours with the hand of one dead of a lingering disease, laid on 'for a pretty while, that the cold might thoroughly penetrate.'

But if we multiplied these instances, we should stray too far. Whether the remedies so eagerly sought and studied, cured rather by faith than by physiology, is of less interest to us just now than the fact that it was largely through Boyle's investigation of them, begun owing to his own poor health and to the discussions of the Invisible College, that he obtained his insight into chemistry and amassed his wonderful experimental knowledge of the subject. It was not very long, however, before he realized that

"Our Vulgar Chymistry (to which our Shops owe their venall Spagyricall<sup>1</sup> Remedies) is as yet very incompleat, affording us rather a Collection of loose and scattered (and many of them but casuall) experiments, than an Art duely superstructed upon Principles and Notions, emergent from severe and competent Inductions";<sup>2</sup>

and accordingly, he began to "consider the art, not as a physician or an alchemist, but as a philosopher."

<sup>1</sup> The word "spagyric" was apparently coined by alchymists out of the two Greek words *σπᾶν*, to draw forth, and *ἀγείρειν*, to assemble; so it is no bad designation for chemical operations.

<sup>2</sup> *Considerations touching the Usefulness of Natural Philosophy*, 1663.



In this rôle, Boyle saw that both schoolmen's and spagyrists' views concerning matter were as houses founded on sand. The schoolmen imagined that it was far more 'high and philosophical' to discover things by the dry lamp of reason or argument alone, unfed by the fuel of facts; and, as he forcibly points out, if they brought forward experiments at all, these were designed not so much to test the truth of their doctrines as to exemplify them for the instruction of the vulgar. In passing, we may remind ourselves that the mere denouncing of this error would leave the culprits quite unmoved, as may be inferred from parallels of this or any other day; but to the newly-arising scientists near him, as to ourselves, Boyle's indictment would have full value. The theory-monger's aloofness from experimental evidence as opposed to deference to authority may be illustrated by Glanvill's tale:<sup>1</sup> a man and his wife had an argument on some occurrence, incredible to the wife, which the man had in fact seen; whereto the lady replied, "And will you believe your eyes, before your own dear wife?"

When the alchymists expounded their views, they were wont to wrap them up in such obscurity of language that even adepts could hardly find out what they meant. They would give their works such titles as "The Philosophers' Dragon which Eateth up her Own Tayle," "Knock the Child on the Head," and similar curiosities. Here is a theory of matter:—

"[The one principal matter] is no other than a mere vapour, which is extracted from the elementary earth by the superior stars, or by a sidereal distillation of the macrocosm; which sidereal hot infusion, with an airy sulphureous property, descending upon inferiors so acts and operates as that there is implanted spiritually and invisibly a certain power and virtue in those metals and minerals; which fume, moreover, resolves in the earth into a certain water wherefrom all metals are thenceforth generated and ripened to their perfection. . . ."

<sup>1</sup> From *Plus Ultra*, 1668.

And here are working directions for an alchymical experiment :—

After our serpent has been bound by her chain, penetrated with the blood of our green dragon, and driven nine or ten times through the combustibile fire into the elementary air . . . ; if this furious serpent does not come over in a cloud and turn into our virgin milk, or argentine water, not corrosive at all and yet insensibly and invisibly devouring everything that comes near it, it is plainly to be seen that you err in the notion of our universal menstruum.<sup>1</sup>

Upon this enigmatic style Boyle pours scorn :—

“ For [he says] in such speculative enquiries, where the naked knowledge of the truth is the thing principally aimed at, what does he teach me worth thanks that does not, if he can, make his notion intelligible to me, but by mystical termes and ambiguous phrases darkens what he should clear up; and makes me add the trouble of guessing at the sence of what he equivocally expresses, to that of examining the truth of what he seems to deliver.”

Later, again :—

“ But if judicious men skilled in chymical affaires shall once agree to write clearly and plainly of them, and thereby keep men from being stunned, as it were, or imposed upon by dark or empty words: 'tis to be hoped that these men finding that they can no longer write impertinently and absurdly, without being laughed at for doing so, will be reduced either to write nothing, or books that may teach us something, and not rob men, as formerly, of invaluable time; and so ceasing to trouble the world with riddles or impertinencies, we shall either by their books receive an advantage, or by their silence escape an inconvenience.”<sup>2</sup>

<sup>1</sup> I have ventured to borrow these two translated examples of “Valentine” and Paracelsus from Professor Pattison Muir’s book, *Heroes of Science* (London, 1883), pp. 15-17.

<sup>2</sup> *Scep. Chym.*

It is not unlikely that Boyle's objections are a little tinged with the disrespect usually accorded by one generation to the style of its near predecessors; nevertheless, it was no transient vogue which he set, for his cautions remain valid to this day: he is a modern.

Let us turn now from Boyle's attack on methods and on exposition to his discussion of results; and we shall confine our attention to one major problem, namely, the composition of matter. Realize first that it is quite impossible here to give an adequate idea of the great mass of observations—made or verified mostly by himself—upon which Boyle bases his views; and bear in mind that when he does allow himself to speculate, it is speculation of the true inductive kind, put forth tentatively, even diffidently, as material for future testing.

The ancients, following the Aristotelian school, named as elements Earth, Air, Fire, and Water: these were called, after the hall where their originators walked, the 'Peripatetic' Elements. The later followers of Paracelsus (1493-1541) partly added to these, and partly substituted for them, three 'Spagyric Principles,' 'Hypostatical Principles,' or '*tria prima*,' namely, Salt, Sulphur, and Mercury. In Fire, the spagyrist discerned the grand agent for resolving bodies into their three Principles. Some workers had sought a yet more potent agent or solvent for breaking down any material into its ultimate constituents; of this nature was "The Alkahest" which was sought by the famous van Helmont of Louvain and Vilvorde (1577-1644). But a more numerous body of alchemical philosophers held to the hypothesis, descended from Aristotle by devious steps, that all matter was made of one and the same stuff, endued in different substances with varying *Qualities*, which were supposed to be, as it were, plastered on to or impregnated in the primordial vehicle of them; and these men imagined that if they could find the 'Philosophers' Stone,' it would prove a potent agent for stripping bodies of their enveloping qualities, so as to leave pure and noble matter,



invested with only such remaining Qualities as constitute gold.

Now, it is most important, if we are to understand rightly the story of chemical ideas before, during, and for long after Boyle’s day, that we should have the doctrine of discrete Qualities clearly in our minds. For chemistry was fighting its way out of the thickets of that doctrine until the end of the eighteenth century. And although there were many stumblings and backslidings before it finally broke free, it had gained the project of escape, first conceived as a set plan by Boyle, who while he lived led it without falter along the only possible track.

The early philosophers, then, looked upon the qualities or properties of a material as being additions to rudimentary bare matter. An object is blue, according to this doctrine, because it has Blueness in it; the property is a sort of decoration conferred upon its wearer, and the Blueness is *independent* of the other Qualities which may have also been put there. The total sum of Qualities thus forms a mask behind which lurks the central being, Matter. (The very word “substance”—that which stands below—embroils us with the doctrine to this day). The various sorts of Quality were classified according as they were (in modern phrase) necessities or ‘extras’; a body might be divested of certain of its Qualities without losing its nature and the right to its name, whereas certain others had to be left *in situ* if, for example, gold were to continue to be gold. All this, which is a theory of man-given *names*, was then applied as if it were a theory of *things*.

From this it was no great step—and the alchemists took it—to assign a few of the more remarkable Qualities especially to a few of the typical varieties of matter; and to go even further astray by *restricting* the possession of a given Quality to that one variety of matter in which it most evidently resided. In this way Sulphur became the Principle of Inflammability, and so forth. These arbitrarily-chosen “Elements” or “Principles” were thought of as vehicles for the



conveyance of their allotted Qualities into the bodies which they helped to constitute; and conversely, the presence of any one of these Elements could be diagnosed in a body by recognizing there the relevant Quality. And, mixed up with all this, there still remained the notion, that Qualities not only reside in a thing, but may be extracted from it or added to it piecemeal: so the last and most wanton step in the argument was, that to succeed in conferring a given Quality on a body is tantamount to forming the corresponding material Element in it.

It was upon this philosophizing that the alchemists, in the childhood of science, based their claims to the feasibility and achievement of transmutation. If only, they said, we might so work upon our raw material as merely to add the desirable Qualities, we shall have something which is "nearly gold," and if we can also take out the unwanted Qualities that still spoil its nobility, we shall have perfect gold.

An actual illustration will best convey what has been said; and as it is taken from a successful text-book of 1727,<sup>1</sup> it will show how doggedly the doctrine persisted; indeed, even at the present day, the technical terms and a trace of the doctrine linger on in many of our most useful words and phrases, in addition to the example mentioned in a former paragraph. (The symbols shown in the quotation are of high antiquity, and used to be connected with astrological and mythological signs.)

"☿ Quicksilver evidently shows gold [☉] in the middle, or body of it, silver [☽] at top, or in the face, and a corrosive [+] at bottom; accordingly, all the adepts say of mercury, that it is gold at heart, whence its heaviness; that its outside is silver, whence its white colour; but that there is a pernicious, corrosive sulphur adhering to it, denoted by the cross; that if its brightness and its

<sup>1</sup> Boerhaave's *New Method of Chemistry*: Shaw & Chamber's English edition; Longmans, London, 1727.

corrosive could be taken away, it would remain gold : that the quantity of sulphur is here so great, as to render it wholly combustible by fire ; that the more 'tis burnt, the nearer it comes to gold ; and that were it perfectly calcined and purified, and its colour changed, it would be gold. . . . And hence that maxim upon mercury : Strip me of my clothes, and turn me inside out, and all the secrets of the world will come forth."

At earlier times such explanations of the theory took a more purely symbolic form, by reference to births, deaths, and marriages, wild beasts, and the whole stock-in-trade of anthropomorphism ; but the underlying notions, except when now and then the symbolism was allowed to run away with them into utter mysticism, do not appear to have differed from those that have here been summarized. The whole doctrine, and the allegations of success, of alchymical transmutation, thus rested not only on defective observation, but also upon the topsy-turvy notion that a particular substance is being generated when qualities like its own begin one by one to appear.

In the book called *The Origin of Forms and Qualities* Boyle brings together his clear-cut views on the doctrine of transferable Qualities which was summarized on pp. 63-64 ; and in so doing, he performs an office which seems to be especially acceptable to English and Latin minds, namely that of seeking causes in concrete objects, solid bodies, determinate things, in place of *ad hoc* abstractions. He adopted the basic principle that the qualities of an inanimate body are not anything inhabiting it, but are simply descriptions of what happens to it when it meets other bodies or external agencies (light, etc.), and of what it does to these other reagents. A property is a mechanical response to, and an effect upon, outside agents ; and Boyle does not omit to include among such reagents the organs of sense, which are affected so as to give us perception. Thus, a thing is blue, not from an inherent Blueness, but

because of the way in which its physical texture passes on light to our eyes.<sup>1</sup> And the same mechanism—the detailed structure and inner motions of the matter, which together are what compose the particular substance—is responsible for *every* effect produced by or upon that substance. If any of the effects are missing, then the mechanical structure is different, or the inner motion, or both. Exactly what Boyle meant by “structure” is reserved till later in the chapter.

No doubt, anyone who could truthfully refuse outright the ‘mechanistic’ view (which now is our own) would have taken up a position quite impregnable; for then any appeal to experiment would have been fruitless. But nobody is quite immune to arguments of the senses; and the spagyrist were not at all so, for although they were cruelly smothered by the *débris* of unreal formularies, they were struggling to reach open air by way of experimental work and hope. Consequently, Boyle was able to appeal to them by producing experimental evidences; and we may briefly review his more cogent proofs that Salt, Sulphur, and Mercury, with or without Earth as well, are *not* primitive Elements, and that Fire is not instrumental in so exhibiting them.

Boyle, specifying experiments of his own and quoting others in support, shows how the heat of the fire does *not* resolve all bodies into components. For instance, a piece of gold lost no weight through being kept in the furnace; silver was reported to have lost a little, but there was no proof that this arose from the expulsion of any “sulphureous” ingredient, rather than from the simple evaporation of the metal itself.

Next: even when the body which is heated does suffer decomposition—for instance, if wood is heated—we have, says Boyle, no right to assume that the resulting products were present *as such* in the original wood. (To borrow a modern simile

<sup>1</sup> The finest experimental proof of the correctness of this teaching is given in the classic letter of Isaac Newton, written from Cambridge on 6 February, 1671/2, for communication to the Royal Society, in which he announced the nature of white light and colours.



from Dr. Aston : a pistol produces smoke, but we do not say that it, or the cartridge, contained smoke beforehand.) So that, even if salt (for example) is found to remain when wood is reduced to ashes by the fire, it does not necessarily follow that the salt had been there all the time; it might have been formed chemically, by the conjunction of other substances present in wood, and in fact this is what the available evidence suggested; and so its presence is not in itself a proof that salt is a simple primitive element. For, says Boyle, I “ allow nothing to be an element that is not perfectly homogeneous ”; <sup>1</sup> and again :—

“ I now mean by elements . . . certain primitive and simple, or perfectly unmingled bodies; which not being made of any other bodies, or of one another, are the ingredients of which all those called perfectly mixt bodies are immediately compounded, and into which they are ultimately resolved.” (*Op. cit.* p. 187.)

He shows that a compound which decomposes may stop short of being dissevered into its ultimate elements, and can merely yield other compounds of these, and he makes the converse point that a compound may be formed from the union of two bodies which are themselves compound. This will be discussed again below.

Further, Boyle shows, the products of such actions of the fire on wood and other bodies are not, as a matter of experimental fact, limited to three kinds of substances; and finally, they are commonly not in the least really like sulphur, salt, or mercury. The truth is, he says, that when the chymists

“ anatomize a compound body by the fire, if they get a substance inflammable, and that will not mingle with water, that they presently call sulphur; what is sapid and dissoluble in water, that must passe for salt; whatsoever is fixed and indissoluble in water, that they name

<sup>1</sup> *Scep. Chym.*, “ Everyman ” edition (Dent, London); p. 132.



earth. And I was going to add, that whatsoever volatile substance they know not what to make of (not to say, whatever they please) that they call mercury."

The error, moreover, is not simply a question of conventional nomenclature :—

" they might freely have called anything their analysis presents them with, either sulphur, or mercury, or gas, or blas,<sup>1</sup> or what they pleased,"

but they must not use one of these names to cover what are, in actual fact, substances of very widely differing natures. Here it is evident that Boyle has met one of Bacon's Idols: he is pleading for a recognition of real distinctions which have been laid bare by the advance of experimental knowledge.

We should note that, despite his emphasis, he is not dogmatic; for he says :—

" I would fain see that fixt and noble metal we call gold separated into salt, sulphur, and mercury; and if any man will submit to a competent forfeiture in case of failing, I shall willingly in case of prosperous success pay for both the materials and the charges of such an experiment."

This seems to have been the only bet which the gentle Boyle was ever drawn into. And he goes on, using an argument which none but an understanding chemist would have the right to use against other chemists :—

" For although I dare not absolutely affirme it to be impossible to analyze these bodies into their *tria prima*; yet because neither my own experiments nor any competent testimony hath hitherto taught me how such an

<sup>1</sup> " Gas " and " blas " were words artificially coined by van Helmont: they meant much the same, but the latter has never gained currency.

analysis may be made, or satisfied me that it hath been so, I must take the liberty to refrain from believing it, till the chymists prove it, or give us intelligible and practicable processes to perform what they pretend."

Finally, in order to show to others how easily inexperience may be misled by superficial resemblances, Boyle actually describes—and explains—a "cooked" experiment, wherein he seems to resolve copper into mercury and sulphur; "but," he adds, "you know I was not cut out for a mountebank."

In all this, Boyle preaches a sermon which is very far-reaching. An untrained observer cannot notice the detailed happenings which together make up any natural occurrence; nor is he aware of the set of tests to which the result or product must respond *in full* before it can be diagnosed and assigned to its true category; what he can take heed of is the obvious alone. In consequence, no explanation which he may attempt can suffice to describe the occurrence; and nothing short of sheer luck, or else an entire absence of competent investigation later, can prevent it from turning out to be completely wrong. For instance, most of us enjoy reading stories of the Egyptian priests' magic powers over matter, or we may please our fancy with the notion that here or there in the past an alchemist realized his dream: such beliefs were held, no doubt, in fair faith by their founders, if merely in wondering ignorance by their later professors; but the dull fact is that the very foundation of such marvels lay in misread appearances. Why we are entitled to say this now, is because the response of inert matter to a vast range of experimental conditions is now known, and the results have proved to us that although during the ages the human mind has very likely varied in its responses to stimuli, inanimate matter at any rate never does so. Only when the results of experiment or art are submitted to *æsthetic* judgment, can we reckon with the likelihood that our forefathers were at levels higher than our own; whereas in 'natural knowledge' the continued imposition of the less subtle but unquestionable

tests of science leaves now no alternative to an advance out of less knowledge into more.

Thus far we have come in clearing up the older doctrines; and we may note that although Boyle's destructive criticisms were directed against what is (thanks to him) dead and done with now, yet they have in them a great deal that finds application always. What did he put forward to replace the ill-founded ideas that he had assailed?

Boyle had, and habitually used, a *corpuscular* notion of matter; and to a very large extent it was derived from and supported by his own experiments, so that in making use of it he felt a degree of confidence that he did not gain from book-study of the Epicurean atomic ideas, which his own seemed to resemble. Indeed he emphasizes the fact that the ancient atomic philosophy, though it was an "intelligible" one, "seems hitherto not to have so much as employed, much less produced, any store of experiments. . . ." It is doubtless unnecessary to explain that he did not and could not establish an atomic theory in a form amenable to quantitatively precise testing, for he had not the facts upon which such a theory could be framed. He made a guess as wide and as free from arbitrary assumptions as it could consistently be, found it an inspiration as a working hypothesis,<sup>1</sup> and exploited it with experiments as far as he could; and his interpretations of results are clearest to us when they are expressed in terms of it. It is easy, however, to misconstrue and to put a too-modern gloss upon many of Boyle's conclusions, and at the same time to fancy them inconsistent with some of the rest, unless we grasp the nature of his atomic views, and unless also we realize that he did not happen to use the word "element" as we now use it in chemistry, but nevertheless discovered the *thing*, with the help of his hypothesis.

First: "it far exceeds the power of merely natural agents . . . to produce anew, so much as one atome of matter, which

<sup>1</sup> *Op. cit.*, p. 85.



they can but modifie and alter, not create";<sup>1</sup> which is, as Boyle says, a truth long regarded as obvious, though perhaps we ought rather to say it was taken for granted. Francis Bacon had put it:—<sup>2</sup>

"There is nothing more true in nature than the twin propositions, that 'nothing is produced from nothing' and 'nothing is reduced to nothing,' but that the absolute quantum or sum total of matter remains unchanged, without increase or diminution."

The second preliminary point refers to Heat: the true and genuine property of heat is, says Boyle, to set moving and so to dissociate the parts of bodies, and subdivide them into minute particles. To the extent of connecting heat with motion, Francis Bacon had reached a kindred conclusion; Boyle's is, in greater detail, remarkably like our own. He went still further—and perhaps not so very far astray—in treating of "the little nimble atomes of fire"; but these were rather the agents of heat than the manifestation of it. We shall meet them again.

Turn now to his corpuscular idea. This seventeenth-century hypothesis is best understood if we utilize, for descriptive analogy, twentieth-century discoveries. Boyle's hypothetical 'Elementary' atoms correspond, not with our chemical-elementary atoms, but with our actual protons and electrons; *i.e.*, they are for him the ultimate stuff and the ultimate units. And he conceived that they were all alike—that is, that there is only one truly primitive Element, only one kind of ultimate corpuscle. What we now call atoms of chemical elements were, accordingly, to him "little clusters" or "little masses," each composed of the ultimate corpuscles; and his own experiments taught him to think that each distinct substance was composed of its own characteristic kind of little clusters. (Here again we recall a modern simile; for us,

<sup>1</sup> *Op. cit.*, p. 63.

<sup>2</sup> *Nov. Org.*, II. Aph. XL.

characteristic clusters of electrons and protons form the chemical atoms.)

Now Boyle knew many experiments in which, with the aid of heat, or by treatment with different reagents, the starting-material could be put through the most profound changes and yet was, in the end recoverable in its original state; so that he recognized that each of its constituent little clusters must be very stable, having all its supposed internal corpuscles bound up very firmly together. Thus: he dissolved metallic gold in *aqua regia*, and heated the product so that it vaporized, in the form of red crystals "very glorious to behold," and soluble in water;<sup>1</sup> then taking the yellow solution of these vaporised crystals, he shook it with mercury, whereupon the solution turned colourless and the mercury became yellow; the amalgam was withdrawn, put into a crucible and heated in a furnace to evaporate away the mercury; and out came the solid gold, regained in spite of all these vicissitudes. By this and by numbers of other experiments, Boyle justly concluded

"that some bodies are of so durable a texture, that their minute parts will retain their own nature, notwithstanding variety of disguises . . .";

and again, that two bodies can mingle so that

"though the compound body made up of them may be very differing from either of the ingredients, yet each of the little masses or clusters may so retain its own nature, as to be again separable, such as it was before."

It is these bodies which now we call chemical elements, when the disguises through which they persist are sufficiently numerous; and we owe the discovery of them, as a class, to Boyle and his experiments. It must by no means be supposed that Boyle was able to say, for example, "iron and sulphur are chemical elements, rust and salt and water are chemical

<sup>1</sup> Gold chloride.

compounds, air is a mixture.” If he had, chemistry could have dispensed with the eighteenth century, for it was to use the next 150 years to be able to make such a statement, and its starting point was Boyle’s classification.

The atoms of the chemical elements are—to Boyle hypothetically, to us actually—halting-places in the organization of matter; but they are not the only halting-places. Boyle took some copper, and dissolving it in nitric acid, got from the solution some fine blue crystals. When these crystals were heated, they yielded a distillate of nitric acid (spirit of nitre) and a residue; and “ that the remaining calx continued copper, I suppose you’ll readily believe.” Thus the acid and the copper had both survived the union; the crystals were a compound of the two, and its little clusters were therefore more complex than theirs. One other example has for chemists some rather remarkable features.<sup>1</sup> Four ounces of nitre were *weighed* into a crucible and heated; “ live coals ” were added little by little until no more ignition took place, after which the heating was continued for a while, to expel all volatile matter. The alkaline product (“ fixed nitre ”) was *weighed*; it weighed less than the original nitre, namely, from eight to ten-sixteenths of it in divers experiments.<sup>2</sup> Spirit of nitre (nitric acid) was then dropped upon the product until effervescence ceased and droplets no longer “ skipped out of the glass ”; and the amount of acid needful to arrive at this point was *weighed*. When the resulting liquor was evaporated, it yielded nitre, the original body. Incidentally, it had in it a little granular saline matter, too little to be examined, “ though we suspected it then to proceed from the want of a just or exact proportion betwixt the volatile and fixed parts of the nitre, that were to be re-united.” Finally, with reference to the weight lost by the original nitre when it was deflagrated, it is noted that the weight of acid

<sup>1</sup> *A Physico-Chymical Essay . . . Relating to Salt-Petre*; also *Producibleness of Chymical Principles*, Part I.

<sup>2</sup> 10·9 sixteenths would be the correct ratio of potassium carbonate to potassium nitrate.



needed to re-compose the nitre did not make good the loss, " yet fell not far short of it."

This experiment, defective though it was,<sup>1</sup> and was felt to be by its author, was recognized by him as of high significance, and was luminously discussed in all its bearings. He argued from it that nitre, a definite substance with definite characteristics, is composed of two others, each of which has its own quite different characteristics, shown by a number of tests duly described. Nitre was thus determined as a substance more complex, less nearly elementary, than either the alkali or the acid.

The special conclusion is, as we know now, incorrect. But—as Dr. Johnson was curmudgeonly enough to say concerning women's preaching and dogs' tricks, " It is not done well ; but you are surprized to find it done at all." It is the motive, the rationale, and the method which count, and there is little need here to specify all the accessory facts, the overlooking of which made Boyle's particular inference fallacious ; Joseph Black, ninety years later, was the first who could have suggested any, and the full exposition had to wait for Lavoisier.<sup>2</sup> What is noteworthy in the first place is the recognition that there are graded stages of structural complexity, *chemical* elements and chemical compounds ; and in the second place, that the method of weight was used as the test. Boyle had a dawning idea of a definite (not, however, immutable) combining proportion between the weights of his alkali and his acid, and he measured it ; his scales told him " the just or exact proportion " between the weights of the two reagents which " had been upon their occurrences fastened to one another " when the stage of mixture

<sup>1</sup> *E.g.*, the complete reaction would need from twice to thrice as much acid as was actually used.

<sup>2</sup> Boyle did not weigh the ' redintegrated ' nitre itself ; nor the hot coals, for he looked on them as merely a sort of succession of matches to set alight the nitre so that it more readily lost its volatile parts. The effervescence on addition of acid is assigned to a vigorous commotion of the liquid only, communicated to some extent to the superincumbent air.

was reached at which “there would follow no more ebullition . . . upon the putting in of more spirit of nitre, unless there were added likewise more of the alkalizate liquor.” And he was disappointed that his technique failed to let him regain in full the weight of nitre with which he had begun the trial, and he says :—

“The redintegration (or re-production) of an analyzed body, if it can be accurately and really performed . . . would certainly be very welcome to the embracers of the atomical hypothesis.”

He, or someone else, did not fail to set the Royal Society at this problem, for Oldenburg wrote in July 1668: “At our Society, we are still upon the mixing of different bodies, and of incorporating them into one another to explore the weight of the total, and of the ingredient parts.”

There is no need to exaggerate the stress laid upon the criterion of weight, for Boyle and his disciples did not constantly employ it as a matter of course; qualitative study must precede quantitative, and he did not have Weight in the forefront of his mind as one of the great inevitable weapons, to which qualitative observation must take a subordinate place in tracking matter through its transformations. How could he? Newton’s *Principia* was not to be written for another score of years, and no one before Newton knew what weight means.<sup>1</sup> It was another generation after that, in the year of Newton’s death, before a chemist could easily teach that Sir Isaac Newton and Huyghens had proved that “all corporeal magnitude has just so much reality in it, as weight . . . weight and reality are correspondent”;<sup>2</sup> and even then the fact was only accepted but not made use of.

To return, finally, to Boyle’s view of chemical compounds as a class: the question is significant not simply because one particular man dealt with it, but because his conclusions were

<sup>1</sup> “The weights of bodies towards any the same planet, at equal distances from the centre of the planet, are proportional to the quantities of matter which they severally contain.” *Principia*, Book III.

<sup>2</sup> Boerhaave, 1727.

what numbers of others after him were to dwell upon in their work. Following out the notion that the simple substances of chemistry were made of little clusters of the corpuscles of one primary Element, and that these little clusters proved apparently hard to disintegrate, Boyle saw two interlinked deductions, both capable of experimental testing. One related to the manner of union of simple clusters; the other, to the manner of their parting when a compound is broken up. Briefly, he thought that when two simple clusters unite, they may do so by simply intermingling, or by sticking more or less closely together; or, on the other hand, they may, in uniting, pool together all their ultimate corpuscles so as to form a new and larger cluster, the single characteristic cluster of the new compound. The former case means that when the mixture or the compound of the two is broken up again by heat, or by clashing with the particles of a reagent, it will yield up the original clusters. The latter case means that if the large cluster of the compound can be broken up, the original individuals may emerge once more, or else probably there may be a re-shuffling of the ultimate primitive corpuscles, whence there must issue clusters of simple bodies other than the original ones. It will be seen that from *this* standpoint, the production of new simple bodies, perhaps including even gold, was logically conceivable; an immense catholicity of possible products, indeed, seemed indicated.

The practical question then was, to study by experiment a number of various conditions under which such effects, if they exist, may be evinced; and this theme was the inspiration of the greater part of all Boyle's researches. Discarding *a priori* schemes as he had to do, he had no means of telling beforehand which conditions might favour either of the two possible modes of disintegration, nor how far he might not be able to break up even simple clusters by drastic agencies; so everything that he could compass was tried and tested. Naturally, as we are lucky enough to know, the more deep-seated transformations which he sought needed, in fact, forces



more potent than he had at hand; and in his partial understanding of this, Boyle made one remark which those who did command such forces—Sir Humphry Davy in 1808 and Sir Ernest Rutherford in our own day a century later—might take as a friendly message from him:—

“How know we, but that nature has made, or art may make, some such substance as may be a fit instrument to analyze mixt bodies, or that some such method may be found by human industry or luck, by whose means compound bodies may be resolved into other substances than such as they are wont to be divided into by the fire.”

But, greatly though Boyle enriched chemistry with new reactions, new substances, new analytical tests, the results which he considered as favourable to his hypothesis are often illusory. *Ex hypothesi* he drew too vague a distinction between physical mixtures, impure bodies, “magisteries,” and chemical compounds; and despite the exercise of a caution and the application of tests for identity, which went far beyond what had hitherto been used, he was barred out, at the very doors of truth, for want of one key—the constant test of Weight. He himself laid no claim to infallibility, and he ends the *Sceptical Chymist* in the true vein of a pioneer:—

“Perchance the enquiries of others have scarce been more unsatisfactory to me, than my own have been to myself.”

Boyle dragged chemistry out of a welter of sophisms and charlatanry, endowed it with life and ideals and material, and set it forward upon the true path. Chemists can nowadays use terms as brief as “analysis,” “synthesis,” “chemical element,” each of which covers great groups of phenomena; it was Boyle who had to hew out, to array, describe, and to assess these great groups, in order for there to be known anything to bestow names upon at all. And he had masses of new facts to record; rarely has there been so prolific an experimenter,

and later generations were to regard his works as a nearly inexhaustible magazine of material and of practical method, so that for this reason alone, even when they failed to grasp all the purport of his teaching, they looked back to him as a master chemist. His works explore many sciences (to say nothing of his one novel, or of his religious essays), and collected together they fill five large tomes. His pages and paragraphs go billowing seductively along, swelling with parentheses, cautions, qualifications, courtesies to the reader, mild chaff at his own expense, odd turns of phrase; the style seems long-winded, yet it is not too much so for the innovation of thought as well as the exact faithfulness of unprejudiced recording, which were its author's aim.

Nothing has been said here of the "Vacuum Boyleianum," nor of 'Boyle's Law' of the pressure of air; this is his most familiar work, least hidden under later superstructures, and it was a great work, but it is not his greatest. Mariotte, seventeen years later, might rediscover Boyle's Law; but neither Mariotte nor anyone else could have done what Boyle did when in making *The Sceptical Chymist* he founded a science.

## PART 3

### THE SEARCH FOR THE ELEMENTS





## CHAPTER VI

### THE ENGLISH SCHOOL OF CHEMISTRY, SEVENTEENTH CENTURY

FOR a hundred and twenty years after Boyle, the practical theme underlying the chemical work of Europe was the answering and exemplification of the problem bequeathed by him : Which bodies are chemical elements, which are chemical compounds? Until Dalton's epoch, it was a period in which old and new substances were being put into one or other of these two great classes, with the aid of methods only partly known to Boyle when he framed the classes. At the same time, the lesser categories included under the group of elements or under the group of compounds were made increasingly definite, while the recognition of gases as chemical bodies was also a necessary step in the general process. By the end of the eighteenth century the work of classifying had gone so far, and mediæval modes of thought had so passed away, that, although hardly anything was yet known of the chemistry of carbon, there were enough data to call for a general theory. The atomic theory was duly produced, and, since the measurement of precise quantities had by now begun to be a part of chemistry, the theory could be put properly to the test as never heretofore.

The most prominent aspect of the search for elements and compounds during the greater part of the eighteenth century was that involved in the study of combustion.

For the essence of the history of combustion is, that one set of chemists, the upholders of the phlogistic hypothesis, asserted that all combustible materials *must* be compounds; their opponents, both before them and after them, believed and finally proved that not all combustibles are compounds : that combustion is primarily an act of combination between two bodies, not an act of sundering of one.

Thus the chemical study of combustion was fundamentally a special case of the process of assignment of simple or of compound nature to substances in general; and, since com-

bustibles and their products comprise the great majority of chemical substances, it is obvious that this special case had to be settled before anything could usefully be done for the firm establishment of a proper theory of chemical combination.

We shall therefore follow out some of the main lines of the study of combustion.

The name of the Phlogistic Hypothesis is familiar enough; and we are apt to think of it as a wrong-headed notion, adopted in error at the first putting of the problem, and maintained with an almost wilful disregard of known facts, to the detriment of chemical progress. Much of this is true; yet when we look into the growth of the hypothesis, we find that the problem itself was very old, and that the phlogistic answer represents no sudden aberration; it is merely a direct continuance of the ways of thought which Boyle and the English school had striven to counteract. In chemistry in 1700 the continental calendar was half a century behind the English one; Stahl of Halle, the founder of phlogiston though not of the hypothesis, was merely one representative of a generation which survived—as generations always do—rather longer than later historians think quite proper. He inherited the spagyric type of mind, and invented a name to explain a Quality, just as the spagyrist had done, and he proclaimed a dogma no whit different from theirs. There were in actual fact three phases in the study of combustion, which might for conciseness be labelled Boyle's, Stahl's, and Lavoisier's; or, if it is preferred, the English, German, and French phases. It is further the case that Boyle's and Lavoisier's are connected directly, without any logical intervention on the part of Stahl's hypothesis, which really existed long before Boyle's, and lived on independently along with it. Hence, however important the rôle of the phlogistic theory was, its place can be judged only when we know what preceded it as well as what followed it; and we have thus to survey first the features of combustion as they appeared to experimenters of the seventeenth century.



It is not easy, nor is it for our purpose necessary, to dissect the early views of fire, flame, light, and heat. There was much confusion; for example, one thing that particularly puzzled experimenters was the relationship between solar heat, intensified by the burning-glass, and the heat of a fire. It is significant to note that from quite early in the seventeenth century the association of heat and fire with special *fiery particles* was common; and we may see in Galileo<sup>1</sup> an early source of this (1638).

“Sometimes,” he says, “when I have observed how fire winds its way in between the most minute particles of this or that metal, and, even though these are solidly cemented together, tears them apart and separates them; and when I have observed that, on removing the fire, these particles re-unite with the same tenacity as at first, without any loss of quantity in the case of gold and with little loss in the case of other metals, even though these parts have been separated for a long while, I have thought that the explanation might lie in the fact that the extremely fine particles of fire, penetrating the slender pores of the metal . . . would fill the small intervening vacua and would set free these small particles from the attraction which these same vacua exert upon them. . . . Thus the particles are able to move freely so that the mass becomes fluid. . . .”

Galileo, holding that nature abhors a vacuum and that solid cohesion is due to this, was inclined to think of fire as providing particles which acted in a sense as wedges between the small parts of matter; this was merely his passing hint at a mechanical explanation.

Francis Bacon, in the second part of *Novum Organum*, adduced a great number of facts which went to show the association of heat with motion; and Boyle, who was familiar

<sup>1</sup> *Discourse on Two New Sciences*, trans. Crew and de Salvio, New York, 1914.

with Galileo's work and with Bacon's, may have combined their ideas in his own when he describes heat as being that which sets material particles moving, by means of the agitation communicated by the "little nimble atomes of fire."

A few years later John Mayow made great play with these fire-atoms, as we shall shortly see; and both Boyle and Mayow based their hypotheses upon experiments of real cogency. It is not our part here to follow up the history of heat in its physical aspect, however, as much as in its chemical effects;<sup>1</sup> and so we shall note simply that speculations as to the physical nature of heat could not fruitfully develop until instruments for measuring it had come into use. The thermometer of the seventeenth century was not a weapon easy of application; it consisted of a flask, partly full of water, inverted neck downwards in another vessel containing water; and the air in the flask expanded or contracted as the surrounding air grew hotter or colder. The use of mercury or of spirits of wine, as in our present thermometers, did not come in until the eighteenth century; and even so, the standardization of scales was a difficult matter, so that we find Stephen Hales in 1727 fixing one of the points of his scales by plunging the thermometer in "the flowing blood of an expiring ox."

The thermometer measures the intensity of heat, its readiness to pass from one body to another; for the determination of quantity of heat, there were no data before the discovery of specific heat in the middle of the eighteenth century. Thus, to seventeenth-century experimenters, the only instrument of precision, optical appliances excepted, where-with the effects of heat might be studied, was the balance; all that they might do was to decide whether heat in itself has weight or not. We shall see that the question was indeed attacked, but that it became confused with the question of the weight of the chemical products of heat. To the chemical study of burning we now turn.

There is a pleasant tale of Queen Elizabeth and Sir Walter

<sup>1</sup> See, however, the quotation from Boyle on p. 97 below.

Raleigh, which expresses fairly well how combustion appeared to anyone at that time and to everyone (excepting Boyle and Mayow) who studied the subject in two hundred years afterwards. Raleigh was extolling to the Queen the wonderful virtues of the new herb, tobacco; she grew perhaps a little bored by his enthusiasm, so he bethought himself of a trick in chemistry to season his talk. So well, he said, did he know the ways of tobacco, that he could tell her the exact weight of the smoke in any quantity of tobacco to be consumed. The Queen impulsively fixed her mind on the most impracticable way of doing the experiment—by catching the smoke upon the balance—suspected him of humbug, and laid him a wager that he would not do what he claimed. So he weighed out some tobacco, smoked it to a finish in the royal presence, and weighed—the ashes. Her Majesty was unable to deny his explanation that the difference between the weight of the original tobacco and the weight of the ashes must be the weight of what had been evaporated in smoke; and, in giving her royal assent to the Law of the Conservation of Mass, the lady remarked as she paid over the money that she had heard of many a labourer in the fire who had turned gold into ashes, but that Raleigh was certainly the first that had turned smoke into gold!

The simple view that burning consists solely in the expulsion of airy matter or smoke from the ash which forms the permanent part of the fuel remained as the central idea of combustion for many a long day; and as to the flame accompanying the burning, Isaac Newton put it, later on, that “the flame of a body is only the smoke thereof heated red hot; and the smoke is only the volatile part of the body separated by the fire.”<sup>1</sup>

This view made very little distinction between plain evaporation, combustion, and what we call ‘destructive distillation’; and we have already seen how Boyle, in *The Sceptical Chymist*, insisted on the distinction, pointing out

<sup>1</sup> Newton, *Optics*.



that when a vegetable fuel is heated there is a drastic and well-nigh *irreversible* transformation of its inmost constituents. But the older chemists, and indeed some later ones also, were content to describe mercury as "combustible," meaning merely that it flies away in a fume if it is heated.

The next point to be realized is that at the middle of the seventeenth century most chemists perceived some kinship between the burning of fuels and the rusting or calcination of heated metals. If, for example, lead or antimony is heated in a furnace, there is produced a smoke, and eventually all that is left is a non-metallic ash or calx. The formation of ashes and smoke, and this alone, was their justification for inferring that metals are inflammable in the same sense as fuels are inflammable. To understand our history aright, it is essential that we recognize that this conclusion is in fact fairly correct but that it is also quite unwarranted by those particular premises; and the course of the study of combustion between 1660 and 1790 seems inexplicable unless we grasp it, in epitome, as follows: Boyle took up the foregoing premises and conclusion, and laid bare a number of factors in combustion which showed the logic of the argument to be unwarranted. He therefore naturally discarded the conclusion that had been drawn from it. Hooke and Mayow then added further new facts to Boyle's. Stahl in 1698 next attacked the matter from a new angle, and he showed that the conclusion had been correct after all; and, putting aside nearly all the facts that Boyle, Hooke, and Mayow had brought out, he assumed that because the conclusion was correct, therefore the logic whereby his forbears had reached it was also correct; he went back to the old alchemical explanation, furbished it up, and gave it a new name. And the next eighty years found chemists confronted with the dilemma of one set of undoubted facts (Stahl's) at variance with another set (Boyle's); the latter were relegated to the background until certain discoveries and the growing recognition of certain principles thrust them forward again; when, embodied in the work of Lavoisier,

they took their place with all that had been truly expressed in the phlogistic theory. For it must be recognized that the phlogistic theory, invented to explain combustion, turned out to explain almost everything *but* combustion.

Let us now turn first to the discoveries in combustion associated chiefly with the English school of chemists of the seventeenth century. These turn upon three main themes, at first distinct and later intermingled: the material nature of air, its necessity to combustion, and the gain in weight of metals when calcined.

That air is real matter, having weight, was apparently claimed first by Aristotle, who found that a leather bottle weighed more when opened out and inflated than when collapsed. But it was Galileo, early in the seventeenth century, who first gave a true and full demonstration, and there are few better examples of his acumen than the account which he gives of the two experimental methods by which he measured the specific gravity of air,<sup>1</sup> without being able to evacuate the flask used to hold it.

Briefly, Galileo did not try to weigh the air normally contained in a flask of known volume; instead, he forced *more* in, and weighed it and in a very neat fashion measured the volume which the added air took up when released again; paying all due attention to the niceties required by the buoyancy of the vessel in the atmosphere.<sup>2</sup>

It would appear strange that Galileo, with this definite knowledge of the heaviness of air, should have not applied it to explain the mercurial experiment of his pupil Torricelli, or his own earlier puzzles concerning the maximum "lift" of a water-pump; but he found much that was explicable by his

<sup>1</sup> *Op. cit.* 77 ff.

<sup>2</sup> In a very much cruder fashion, the first measurement of the specific gravity of a *vapour* was made by Francis Bacon, who used a bladder to perform upon the vapour of alcohol a measurement of density by a way not unlike our "Dumas" method. See *Nov. Org.*, II. Aph. XL.

new mechanical principles and by the natural abhorrence of vacua, in which he had a strong trust; and so it was left for Boyle to demonstrate the true origin of nature's "abhorrence."

The experiments of Boyle and his assistant Hooke with their air pump were in progress at Oxford in the late sixties, and they had a second and improved pump with which many trials were afterwards made at the Royal Society. No trace of either pump now survives, but Boyle left ample descriptions of their design. The outstanding experiments were those in which fire was seen to die out in a vessel exhausted of air.<sup>1</sup>

This fact was very remarkable; for if the combustion of a fuel were simply the violent emission of volatile matters from it, why should the co-operation of air—as well as of heat—be necessary? Boyle admits that he suspects "that there may be dispersed thro' the atmosphere, some odd substance . . . on account whereof, the air is so necessary to the subsistence of flame. . . . It does seem surprizing what should be in the air, which enabling it to keep flame alive, does yet, by being consumed or removed, so suddenly render the air unfit to preserve flame. . . ."

He made experiments to see whether the volume of air—or rather its "elasticity"—was much altered when a fuel was burnt in an enclosed vessel, but seems to have found very little change. "This undestroyed springyness of the air, with the necessity of fresh air to the life of hot animals, suggests a great suspicion of some vital substance, if I may so call it, diffused thro' the air. . . ."

These observations are recorded in a tract called *Suspicions about some Hidden Qualities of the Air*; it does not appear that Boyle himself found the opportunity to follow

<sup>1</sup> We inherit the contradictory term "receiver" for such a vessel—which does anything but receive—from the accident that Boyle found it convenient to adapt for the purpose those flasks which were shaped for the reception of liquids distilled over from alembics or retorts.



up the idea of a "vital substance," nor indeed is it likely that he could have gone far in it with the use of vegetable fuels or that of breathing animals.

Hooke, however, took up the problem of the rôle of air, in experiments made at the Royal Society in 1679 and in his 'Discourse on Comets' read there in 1682; and some years earlier, in his *Micrographia* (1665) he had outlined results of much the same kind. Between 1665 and 1679 Mayow had published work on the subject, but it does not appear that Hooke had in the interval gained much from him. The experiments are recorded in January, February and March 1678/9.<sup>1</sup>

Elaborating an experiment of Boyle's, which had come in turn from van Helmont, Hooke shows how coals heated in an enclosed space will not burn until fresh air is admitted to them; nor did a piece of charcoal, weighing 128 grains, suffer a loss of more than  $1\frac{1}{2}$  grains in weight on being kept for two hours red hot when sealed up in an iron box full of sand, so as to exclude air. Again, some coals were set burning in a box provided with bellows so arranged as to blow only with the air shut up in the box, thereby circulating it without access of any fresh air from outside; and after a while the fire went out, the air being "satiated." Similarly, air "satiated" by having coals burnt in it would put out a lighted candle lowered into it.

These facts were taken by Hooke to mean that air acted as a *solvent* for the heated combustible matter (much as, for instance, water will dissolve salt), and that there is a natural limit to the amount of combustible matter which a given quantity of air can dissolve. But he went further; for he brought in the fact—fairly acceptable to all who had seen Boyle's experiments—that a substitute for air is to be found in ordinary saltpetre or nitre; fuels mixed with nitre will burn hotly even without air.

Thus, says Hooke, it seems that there is present both in air and in nitre a substance which causes fuels to burn;

<sup>1</sup> See Birch's transcript of minutes, *Hist. R.S.*, Vol. III.; 1756.

"the Air itself is no farther the Menstruum that dissolves bodies by fire and flame, than as it has such a kind of body raised from the Earth as has a power of so dissolving and working on Unctuous, Sulphureous, or Combustible Bodies: and this is the Aerial or Volatile Nitrous Spirit which, provided it be supplied in the body to be so dissolved, as by Fire, will work the same effect, even without Air." For Hooke, the active substance is a part of air, but not the whole of air; it is like, if not the very same as, the substance present in nitre which enables nitre to support combustion; and it works by forming a sort of aërial solution of the fuel; when it is "glutted" or "satiated," the rest of the air is useless for supporting combustion.<sup>1</sup>

The connection between air and nitre was the theme largely developed by Mayow; but it is possible that it was not quite unfamiliar to the inner ring even when Hooke first published it as new in 1665. As far back as 1657 we find Wren, in his inaugural lecture at Gresham College, casually referring to the presence of nitrous particles in the atmosphere; but as his curious purpose in speaking of them was in a discussion of how the shadow on the dial of Ahaz had moved backwards for Hezekiah, their refractive index and not their part in combustion was at issue, and we are left ignorant of his reasons for supposing them present.

It may be noticed that Hooke's explanation of the part played by air in combustion is satisfactory only when it is applied to ordinary fuels, which largely pass into an aërial condition when they are burnt; he does not say definitely whether or not he thinks that the calcination of metals likewise entails the absorption of some invisible portion of them into the air, as the phlogistonists were to contend later. But he was quite familiar with the researches of Boyle on metallic calcination, to which we have now to refer, and we need

<sup>1</sup> For Hooke's knowledge of the kinship of breathing with burning, cf. *Alembic Club Reprint No. 5*, containing some extracts from *Micrographia*.

not stop to enquire further into what, after all, is rather summarily dealt with by Hooke.

Boyle's experiments upon the gain in weight of metals during calcination are numerous, and—although he abstained, in his customary way, from leaping to a final conclusion from them—they were to remain as a formidable set of facts for later generations to try to explain away and ultimately to accept, *minus* the only one of his important conclusions which was definitely wrong.

Using carefully-weighed quantities of tin, zinc, lead, copper, steel, and other metals, Boyle heated them in cupels in a muffle-furnace, and found that in changing to powdery or to glassy calces they all gained very definitely in weight, by amounts which are duly recorded. This gain was quite distinct from the subsequent slow "acquist of weight" that ensued on prolonged exposure of some of the calces to air, due, as Boyle points out, to the absorption of roving particles of moisture from the air.

One conclusion very properly drawn by Boyle from the evidence now before him was that the burning of fuels is not of the same nature as the calcination of metals. "It is evident," he says, "that in some bodies, especially of a metal-line nature, the fire that makes that which they call calcination, does not operate as it does in the burning of vegetables. . . . It is manifest, that there is a great deal of difference between the ashes . . . of metals and of some minerals, where almost the *whole*<sup>1</sup> body is by the fire converted into a dry and heavy powder, and the ashes of incinerated vegetables, which usually leave but a little quantity of earth behind them, in comparison of the matter, which the violence of the fire hath driven away."

He regards the ashes of vegetable fuels as the products of chemical transformation of a part, but only a part, of the original constituents—which is quite true. He ignored, of course, the true fate of the gaseous products.

<sup>1</sup> Italics are mine.



As to the question, later to become fundamental, of the composition of the calces of metals, he has two conclusions which we should note. The one is that, in view of the fact that no weight is lost, metallic calces are "without question" *not* less simple bodies than the metals from which they were formed; and they may indeed be compounds of these metals with the something else which was the cause of the gain in weight.<sup>1</sup>

Again, he says elsewhere in the same connection <sup>2</sup> :—

"Whereas it is commonly supposed, that in calcination the greater part of the body is driven away . . . it seems that they [these notions] are not well framed, and do not universally hold, since at least they are not applicable to the metals our experiments were made on. For, it does not appear by our trials, that any proportion, worth regarding, of moist and fugitive parts was expelled in the calcination; but it does appear very plainly, that by this operation the metals gained more weight than they lost; so that the main body of the metal remained entire, and was far from being, as a peripatetick would think, elementary earth, or a compound of earth and fixed salt, as chymists commonly suppose the calx of lead to be."

Nothing could be more plainly put than this; and we shall later on learn how this statement—the categorical denial of the future phlogistic hypothesis—was avoided by the school of Stahl.

The second of the two conclusions related to the actual substance that had caused the gain in weight on calcining a metal. It might be thought that since Boyle already suspected the presence of a "vital substance" in air which was essential to the burning of vegetable fuels, he ought to have sought in it the cause of metallic calcination also. But, as

<sup>1</sup> *Producibleness of the Chymical Principles*, Part VI

<sup>2</sup> *A Discovery of the Perviousness of Glass*.

we have seen, he had excellent grounds for *not* connecting the burning of fuels and the calcination of metals; and further than this, the experiments which he did make in such a way as to test the matter directly suffered from blemishes. For, in these, metals of various sorts were sealed up and heated. They were not sealed up *in vacuo*, but in glass retorts or between two crucibles luted together; and the air so shut in naturally afforded some small amount of visible and weighable calcination and was partly consumed thereby. In the tests in which the whole retort with its contents was weighed, the mistake was made that after the heating was over, the sealed tip of the retort was opened *before* the second weighing; and since more air was inevitably sucked in to replace that which the metal had in fact consumed, the final weight of the retort and contents became greater than at first. Boyle thus overlooked the fact that in this sort of trials the "over-all" gain in weight had occurred after the heating and not during it, and he was thus obliged to conclude that the matter causing the gain, whatever it was, had gone *through* the hot walls of the sealed vessels. Hence the title of the tract on the "Perviousness of Glass." Not every substance was found able to retain such particles; he often weighed an object both hot and cold and found no difference; but (naturally) the easily calcinable metals retained the most weight.

The experimental proof that heat is not weighable was first given by Boerhaave, half a century later, and afterwards by Lavoisier; and it is quite possible that Boerhaave's detection of Boyle's error may have prejudiced to some extent the acceptance of some of the latter's other experiments on metallic calcination; for it has often happened that genuine facts have been cast aside simply because the inference made from them has been found incorrect.

Boyle was of opinion that the penetrating matter was probably composed either of the corpuscles of fire or those of the fuel used in the furnace. He pictures these as causing a sort of concretion or cementing of the particles of the metals

into which they had entered; thus his view of the relation of calcined lead or litharge to metallic lead might be compared crudely with the relation of sandstone to a pure quartz crystal. The red precipitate of "mercury *per se*" ought in strictness, he thinks, not to be called "*per se*," since these added particles go towards its composition as well as mercury itself.

In the essay on saltpetre Boyle extends his view to the burning of vegetable fuels. For his work, he says, "renders it questionable, whether or no inflammability doth strictly in all mixt bodies require a distinct sulphureous ingredient; and whether or no it may not result from such a contrivance of parts, as that thereby the particles of the concrete are disposed to be set a-moving by the adventitious . . . corpuscles of another body, in such numbers, and with such celerity, as may put them into that scheme of matter which we call flame."

Boyle did not wish it to be thought that he had yet satisfied himself as to these fire-particles and their origin. For his apparatus and materials were now used up, and he was busied with other work; and so he ends his discourse on "The Ponderability of Fire and Flame" with the following suggestive remarks:—

"Whethêr I should have been able by reduction, specifick gravity, or any other of the ways, which I had in my thoughts, to make any discovery of the nature of the substance, that makes the increment of weight in our ignited bodies" [lack of opportunity] "keeps me from pretending to know. But this account will probably excite you and your inquisitive friends, to exercise their sagacious curiosity in discovering what kind of substance that is, which, though hitherto overseen by philosophers themselves, and being a fluid far more subtle than visible liquors, and able to pierce into the compact and solid bodies of metals, can yet add something to them that has no despicable weight upon the balance, and is able, for a considerable time, to continue fixed in the fire."



The end of this period in the story of combustion concerns the work of John Mayow; but, before it is dealt with, a note of caution is necessary. Valuable though Mayow's work was, it played very little part in the growth of the subject, and it needed the light of later discoveries to exhibit its valuable elements. The reasons for its having been ignored are not now very easy to determine; it is, however, worth while to notice that in Mayow's treatise of 1674 he often transgressed the canons of the New Philosophy, by soaring away from the solid ground of his experiments into speculations of a type which was deservedly falling into disrepute. This would have sufficed to bring upon him adverse criticism from the leaders of the Royal Society, and in addition he was a little too confident in urging points in physiology which were contrary to the experience of his seniors. He had entered Wadham in 1661, too late to join the company of the stalwarts; and most of his work seems to have been done in the intervals of medical practice in Bath or in London, so that he stood beyond easy guidance at the hands of the elders of the faith. Whatever his defects were, they roused in Henry Oldenburg, the jealous but amateurish curator of the New Philosophy and its organ the *Phil. Trans.*, enough irritation to procure a decidedly hostile review of Mayow's *Quinque Tractatus*, especially in their medical aspects; and this probably condemned the whole work to neglect. At the same time, Oldenburg wrote slightly of the book to Boyle, who was in truth the only man who could have passed by medical errors to appreciate the real merits of Mayow's chemical observations.<sup>1</sup>

Within a few years Mayow was elected into the Royal Society, upon Hooke's nomination; but he died only a year later (1679), aged forty-four, and thus had little chance to teach

<sup>1</sup> "I hear some very learned and knowing men speak very slightly of the *Quinque Tractatus* of J. M. and a particular friend of yours and mine told me yesterday, that as far as he had read him, he would shew to any impartial and considering man more errors than one in every page."—Oldenburg to Boyle, 10 July, 1674. This friend sounds like the anonymous *Phil. Trans.* reviewer.

the discoveries which Oldenburg had been so unluckily instrumental in suppressing.

In the treatise *De sal-nitro, et spiritu nitro-aereo*,<sup>1</sup> Mayow lays stress on experiments which, like those of Hooke, show the presence of a constituent common both to air and to nitre, that acts on heated combustibles so as to cause fire, and is also the vital agent in respiration. He definitely conjoins metals with ordinary fuels and with inflammable substances like sulphur. This constituent, "nitro-aërial spirit," is shown to be only a part of air, in some experiments which have been ranked as the beginnings of 'pneumatic chemistry.' In some of these, a candle or other fuel was set alight in a volume of air enclosed in a jar inverted over water, and it was shown that the volume of air diminishes as a result. Two comments are, however, to be made concerning this experiment. The first is the purely historical point that it was an old experiment long before Mayow's time : Francis Bacon knew it, and says <sup>2</sup> :—

"To this opinion [that hot air escapes through the pores of vessels] men have been led by the *common experiment* <sup>3</sup> of an inverted cup placed on water with a candle in it or a piece of paper lighted ; the consequence of which is that the water is drawn up ; and also by the similar experiment of cupping-glasses, which when heated over the flame draw the flesh. . . . But this opinion as to the cause is altogether a mistake. For the air is not diminished in quantity, but contracted in space ; nor does the motion of the rising of the water commence till the flame is extinguished or the air cooled. . . ."

So that, as so often happens in other fields also, the author of the first experiment in pneumatic chemistry is none of those to whom it has been credited ; Priestley's methods derived from those of Cavendish, Brownrigg, and Hales, Hales' from Mayow, Mayow's perhaps from Bacon, and Bacon's from numer-

<sup>1</sup> Alembic Club Reprint No. 17 is a translation of all five treatises.

<sup>2</sup> *Nov. Org.*, II, L.

<sup>3</sup> Italics are mine.

ous humble persons unknown; each in turn improving or simplifying what he had inherited.

The second comment is of greater scientific bearing; how was the shrinkage in air-volume to be explained? Bacon had merely stated the fact of shrinkage, without any sufficient explanation; we say now, that one of the gases present in the air has been removed, in the form of compounds absorbed in the water; but this was not Mayow's explanation, nor would anyone else of his time have thought of it in such a way. To Mayow, as to Boyle, a contraction produced in a given sample of air was due to some change in the structure of *each* of the particles of which the whole sample was composed; not to the removal or using-up of some constituent gas. Boyle's notions as to the structure of air, which produces a resistance to compression, had been expressed by him in two ways: one, by drawing a rough analogy with a pile of springy wood-shavings or flocks of wool or metal springs, that is to say, the air was figured as a mass of springy particles in contact; but the connection of heat with expansion and with motion led him also to a suspicion which contains the germ of our modern kinetic theory of gases—"I will allow you to suspect," he says, "that there may be sometimes mingled with the particles that are springy . . . some others, that owe their elasticity, not so much to their structure, as their motion, which variously brandishing them and whirling them about, may make them beat off the neighbouring particles, and thereby promote an expansive endeavour in the air, whereof they are parts."

Mayow's view was akin to the former one of Boyle's; he took it that ordinary air is a congeries of particles *each* of which has rigidity or resistance to deformation, and that when the air shrinks on being used to support combustion, it is because each of its particles becomes more flaccid, yielding to the external pressure. This idea was one which persisted on to Priestley's day. And the cause, Mayow says, of the initial rigidity of an air-particle is that a nitro-aërial particle is wedged in it; when the wedge is knocked out, the residual particle falls



together and becomes less rigid. In a chapter of rather loose argument, Mayow seeks to ascribe rigidity in almost anything to the presence of a cement of nitro-aërial particles.

Turning now to the action of the fuel upon the air (or upon nitre), the function of the fuel is, says Mayow, that its particles are suitably formed to knock out of air-particles, or out of nitre-particles, the nitro-aërial wedges, which are thus caused to fly about with great speed. And flame, or rather light, is nothing other than these same nitro-aerial particles in a state of vigorous motion. Indeed, Mayow goes so far as to extend this to the sun's light, and he practically resuscitates the ancient doctrine of a heavenly zone composed of the Element of Fire. But, coming to earth again, he recognized in an entirely admirable way what is the fate of the burnt fuel. Thus, the calcination of antimony entails the entry into it of nitro-aërial particles; and Mayow knew of the increase of weight attending this, and assigns it to these particles. Again, when sulphur is burnt, its particles are altered in such a way by the nitro-aerial particles whose agitation it has caused, as to be made sharp and acid: and if the sulphur has been compounded with iron, as in pyrites, the acid into which it is turned attacks the iron to yield vitriol, the product of corrosion of pyrites. In this and the allied parts of Mayow's work, the properties of nitro-aërial spirit are nearly those which Lavoisier embodied in the name oxygen, the acid-producer.

In his understanding of the chemical nature and exchanges of salts and acids, Mayow was ahead of most of his contemporaries, and he recognized the displacement of one acid by another in union with a base. Thus, of the action of hot sulphuric acid on nitre, which sets free nitric acid, he says "no doubt it is because the volatile acid salt of the nitre has been expelled from the society of the alkaline salt by the more fixed vitriolic acid, that the acid of nitre, now liberated from union with the alkaline salt, ascends under a heat no greater than what is required for the rectification of the spirit of nitre"; which, as he points out, will not happen unless the vitriolic acid is

present, to form a compound correctly identified with vitriolated tartar (sulphate of potash).

The most striking of Mayow's experiments, in which he made the soundest advances, were perhaps those in which he prepared and handled gases; many of his devices were very neat, and he made and examined gases which we know now to have been hydrogen and nitric oxide, and which he found to be like ordinary air in several respects, but not capable of supporting life.

It has not been thought necessary to devote much space here to Mayow's successes, for the reasons of his failure furnish a more illuminating comment on the spirit of his day. Enough has perhaps been said to indicate that he was an ingenious experimenter and a clever assimilator of contemporary discoveries, but that he could not grasp the chief of all the new ideas: in making inferences, if an inference is (as Bacon said) larger and wider than the facts whence it is derived, it is insecure, and is not ready to be proclaimed, until it has been put to the proof of a correspondingly large and wide range of facts.

## CHAPTER VII

### THE GROWTH OF PHLOGISTON, XVII-XVIII CENTURIES

WE come now to the Phlogistic Hypothesis of combustion. It will have been noticed already, in the quotations made from Boyle, Hooke and Mayow, that the property of inflammability had for long past been associated with the presence of a hypothetical substance connected with the ancient Element of Fire; the Principle of Inflammability, usually termed 'Sulphur'; and it was a chief part of Boyle's permanent contributions to chemistry to show the errors of such a doctrine, which was the product of those mediæval notions of Qualities which have already been expounded in Chapter V. Yet still, despite the Sceptical Chymist and all that his school could do, and despite the direct denials reviewed in Chapter VI, the old heresy persisted; and its facile attractiveness was enhanced when new names were bestowed upon its central feature. What earlier spagyrist called 'Sulphur' was by their later representative Beccher (1635-1682) re-christened the "Fatty Earth," by his more ingenious successor Stahl (1660-1734) "Phlogiston," and by Boerhaave simply "Oil"; fundamentally there was no difference between any of these things, and one and all were really no more than synonyms for "The Quality of Combustibility."

To judge from his own style of writing, Stahl was incapable of appreciating any work as modern as that of the English school of a generation or two before him; but the boldness and the speciousness with which he insisted on explaining his one real discovery according to principles that should have been obsolete, won over many like-minded supporters in his own country to worship the golden calf of mediævalism. And in England after Boyle's death there was a relapse in chemical investigation, resulting from the concentration of scientists upon the grand physical discoveries of Newton; so that, although Newton himself did much to foster corpuscular ideas, Stahl's notions slipped in and became firmly rooted. Moreover, the first half of the eighteenth century found nowhere any



chemical worker of first-rate merit, but there was springing up a considerable body of dabblers; and this inevitably served to give currency to superficial theories. Listen to Marggraf's complaint, written in the seventeen-fifties, near the end of the barren period :—

“ The physician, the statesman, the financier, the barber and the army surgeon, the brewer and the brandy-maker, the dyer, the tanner, the wise-woman, the charcoal-burner and the woodcutter; yea, even the company-promoter (O! What a sorry label!) are brazen enough to count themselves among the chemists.”

In 1709 it was still not unprofitable in Germany to publish books on the Three Elements whereof Heavens, Worlds, Seas, and all their Visible, Audible, Palpable, Tangible, and Gustible Forms Consist, and it was even possible for such a work to be dedicated to the ‘ Royal Prussian World-renowned Society of Sciences.’ But the books which chiefly disseminated chemistry, which were widely printed and translated, and went through edition after edition, were the text-books of Lemery at Paris and Boerhaave at Leiden. Such works were perhaps not more conservative in their presentation than a text-book is expected to be; the fact remains that although the generation of which they give us a measure had not yet caught up with the standards of Boyle, it was at least given the chance of realizing chemistry as a whole science, for here was the subject systematically and interestingly framed and clearly put forth. The merit of Lemery's and Boerhaave's books was that they prompted numbers of their readers to enter the field which lay open for study. From this cause, as well as for utilitarian reasons, there must have been (at a guess) twenty chemical experimenters in 1750 for every one in 1650. Most of them, it is true, were quite humble operators, and much of the work was ancillary to medicine or to mining and metallurgy; but their collective labours added a good deal to the store of practical knowledge

in the subject even if such theories as they perpetuated were defective and carelessly regarded.

What was the one discovery which distinguished Stahl's work, and which set "Phlogiston" on a more stable foundation than had been given to "Sulphur"? It was simply the recognition of the dual fact that the Quality of Combustibility is present both in fuels and in metals, because it can be transferred from the former to the latter.

A great part of Stahl's work is founded on that of Beccher, who has already been mentioned by name; and Stahl, in republishing Beccher's *Physica Subterranea*, supplemented it with an exposition of his own teaching. Much of this is tedious stuff and full of unsatisfying dogmatic argument, and sometimes misrepresentations; in the end, however, with his *Documenta*, he outlines the experiments out of which his hypothesis has been hammered—expressing the facts always in the language of the hypothesis. Copper, iron, tin, lead, and antimony, when heated in the open air, fall into ashes; or if they are heated as powders with nitre, they are burnt up, sometimes with a spouting flame. "These metals, having thus been burnt, cannot be turned again to their own metallic guise by any experiment or additament except such as can again impart to them, and insinuate into them, the Inflammable Material. Such a thing being applied, they regain forthwith their complete body, metallic fusion, ductility, dissolubility," etc. "Of each and all of which properties they had hitherto been deprived, by *subtraction* of that inflammable material." The materials which are thus able to convert calces into metals include coal and charcoal, and fatty and oily substances; in which the ability to burn is the primary evidence that they themselves contain the Inflammable Principle. In such fuels, says Stahl, "There is conjoined in the very substance of the 'mixt,' as an ingredient, as a material principle, and as a constitutive part of the whole composition, the material and the principle of fire (not fire itself): I myself began to call this *Phlogiston*; it is in fact the *primum ignescibile, inflammabile*,

the Principle immediately and especially fitted for taking up and supporting heat, that is to say if in any 'mixt' it is conjoined with other principles."

And so we can summarize the core of Stahl's discovery as the reaction



It seems a simple enough statement, yet the participation of the fuel needed stating; for example, Boyle had stated that "by a particular way of ordering the fire" of spirit of wine, he could convert calx of lead into metallic lead; he thought that heat alone had done this, not realizing that the agent was some incompletely-burnt spirit in the flame which he used. It is difficult to know how Stahl regarded the exceptional case of mercury, for its calx needs no admixture of fuel to be converted into metal by heat.

For conciseness, let us put the facts and the hypothesis in the form of equations:—

Because



Therefore

$$\begin{cases} \text{Fuel} = \text{Phlogiston} + \text{other principles, and} \\ \text{Metal} = \text{Phlogiston} + \text{calx.} \end{cases}$$

Metals are accordingly compounds of phlogiston and calx; combustible fuels are rich in phlogiston; in heating together fuels and calces the phlogiston is transferred from the fuel to the calx, to constitute the metal; a metal or a fuel when heated parts with its phlogiston, which is taken up by the air, and ash is left behind.

Regarding sulphur (true sulphur), which on being burnt yields an acid, Stahl naturally retained the old view that the acid had been in the sulphur as one of its constituents, the other constituent being the inflammable principle whose escape during combustion left the acid revealed:





This had already been strongly contested, if not perfectly disproved, by Boyle and by Mayow nearly half a century before; yet Stahl alleges the very experiment made by Boyle and before him by Glauber—that of converting sulphuric acid into sulphur by heating it with turpentine—as proving the exact opposite of what Boyle had considered it to indicate. The compound nature of sulphuric acid and the simpler nature of sulphur were upheld by Boyle as probable, the converse was by Stahl asserted as a fact.

It should now be clear how the phlogistic hypothesis is one phase of the larger question, which substances are simple and which are compound? That the hypothesis dogmatically answered the question in a completely wrong way;<sup>1</sup> and that Boyle had not been able to have finally *proved* this wrong beforehand, although he went far further towards doing so than Stahl did towards exhibiting it as right: the reasons for these are instructive, as showing in a simple way how the aspect of a problem may be completely different for two people neither of whom see it all. The key-issue in this case is, what part does air play in combustion? We are less interested in what Stahl personally thought about it than in the attitude of the generality of chemists; and so it becomes worth while to add to what has already been said of the work of Boyle, Hooke, and Mayow, an examination of the way in which the difficulty was met by Stahl and by others also before and after him.

The fact that air is essential to burning was proved, as we saw, by Boyle's experiments with his air-pump. But to show that it is "essential" is not at all the same as proving that the material of air combines with the fuel; we have already seen how Hooke viewed its function as that of a physical dissolvent of the vapours escaping from the fuel; a view to which Priestley at one time inclined a hundred years later. And it had already been observed that although gunpowder has in it *all* the necessary constituents to yield fire, yet it burns very much less freely

<sup>1</sup> In respect of metals, sulphur, phosphorus, carbon and their combustion-products.

*in vacuo* than in air; and the reason for this had been assigned, fairly correctly, to the fact that when air is present, communication between the grains of powder is better maintained than *in vacuo*, so that the fire passes the better from grain to grain. In the same way, it was argued by many chemists, may not air act in helping the burning of a fuel, merely by maintaining touch between its finest parts?

To both of these explanations a reply was already actually at hand, provided only that it was recognized that when metals are calcined, they burn. For in all such cases, as we have already seen, metals were known to increase in weight on calcination. Boyle, unluckily misled, did not learn that it is union with air which causes the increase; Mayow did realize it, though not quite correctly. But, before the time of either of these, the fact had been hit upon, and an interpretation of the part played by air had been made, which remained as the stock explanation for Stahl and for most of his successors for a long time, until the original fact of gain in weight had fallen into disregard altogether: this interpretation was that of Jean Rey, in 1630.<sup>1</sup>

Rey, a French physician, was set a "poser" by his acquaintance Sieur Brun, Master Apothecary of Bergerac: why does tin gain weight when it is calcined? And so Rey took thought, and explained it all, by analogies and *a priori* arguments unsupported by direct experiments of his own, and bombastically set forth; yet his shrewdness led him to make a surprisingly good shot from a very unsound standpoint. Rey's first chapter states "That all things material under the canopy of Heaven are heavy." He says, "The weight of a thing may be examined in two ways, namely, by the aid of reason, or with the balance"; and he goes on, "With the arms of reason I boldly enter the lists to sustain that the weight with which each portion of matter was endued at the cradle, will be carried by it to the grave." He knows that air has weight; and discusses various aspects of weight. Then, to the query of Sieur Brun, his reply

<sup>1</sup> See Alembic Club Reprint, No. 11.

is, "To this question, then, I respond and uphold proudly, resting on the foundations already laid,

" 'That this increase in weight comes from the air, which in the vessel has been rendered denser, heavier, and in some measure adhesive, by the vehement and protracted heat of the furnace; which air mixes with the calx (frequent agitation aiding) and becomes attached to its most minute particles; not otherwise than water makes heavier sand which you throw into it and agitate, by moistening it and adhering to the smallest of its grains.' "

His readers, having now been "tamed and rendered, as it were, tractable by the evident truth of the preceding essays," are presented with objections—some valid, some not—to other explanations; and at last he gives to Chapter XXV the sounding title:—"By a single experiment all opinions contrary to mine are entirely destroyed." Those good old days! As a matter of fact, he is fairly correct; for the experiment, done by one Hamerus Poppius and described in his work called *Basilica Antimonii*, avoided all risk of contamination with smoke by using a burning-mirror to calcine antimony, and an increase in weight had been found.

The crucial point here is that the absorption of air is conceived as a *secondary* effect of calcination: the air "mixes with the calx," entering the pores of the body which calcination had already produced. And this is exactly how the matter was looked upon by all phlogistians who troubled themselves about the matter at all. Stahl, whose sentiments are a little hard to discover, directs attention to the minute pores that must exist in many bodies, and says, "There, at all events, the result is that air idly occupies the empty place thus provided; but one cannot argue from this that it [air] will be necessary to the aggregate, nor does it appear, when the action has taken place, that it [the absorbed air] is needful or essential towards maintaining or sustaining the aggregate itself."

The phlogistians admitted that air seemed to play a useful



part in taking up—as they said—the phlogiston from a fuel; but they were encouraged to neglect it as a prime factor, and to ascribe its absorption in calcination to secondary happenings, by reason of experimental deficiencies. Thus, with the then air-pumps it was not possible to exhaust a vessel so thoroughly that a metal would show no sign of calcination on being heated in it; and it was known that phosphorus glows even more brightly under reduced air-pressure than under high pressure. These apparently exceptional cases were seized upon, and led the all-too-human phlogistians to ignore more and more the rôle of air and with it the gain in weight during metallic calcination. Of the gain in weight on burning vegetable fuels they knew nothing, and it was not until the phlogistic hypothesis was near its end that alcohol was found to burn so as to form more than its own weight of water.

In a subsequent chapter the fate of the air which supports combustion will be touched upon again; meanwhile, we may notice some explanations, other than Rey's or Stahl's, of the gain in weight of burnt metals: explanations which denied to atmospheric air any part in the composition of the calx. Boerhaave, knowing that "fire-corpuscles" have no weight, was inclined to put the gain down to the absorption of parts of the fuel employed. This neglects the experiments where a burning-glass supplies the heat. The other explanations nearly all invoked Archimedes' principle; for instance, Kunckel von Löwenstiern, who made confused and inaccurate tests with antimony, thought he had shown that the calx was heavier than its metal because it was less voluminous, and was therefore less buoyed up by the atmosphere, while being weighed, than the metal had been. This is, of course, untrue, for calces are less dense than their metals: Boyle had found this with some precision, but this inconvenient fact was neglected by Kunckel. By a few continental chemists the idea was put forward that phlogiston, present in metals but not in the heavier calces, has a negative gravity; but no one of any consequence was prepared to place quite such whole-hearted confidence in

the powers of phlogiston. Towards the end of the period Guyton de Morveau advanced the idea that phlogiston might be simply a very light substance, lighter than air, and so it would buoy up the calx which contained it when constituting the metal, if this were weighed in air; much as a dry sponge, full of air and weighed in water, weighs less than after the air has been caused to escape from its pores. This too had to go, specious though it was, in the face of the quantitative facts soon thereafter to be shown.

Only the weighing of *all* the materials involved in a combustion, a calcination, or in the reduction of a calx, could establish the primary function of air; and even so, the nature of gases as individual substances had first to win recognition. Until these two things happened, the phlogistic hypothesis held the field, in virtue of the causes already indicated. It was at first a useful aid in grouping certain phenomena; next it was to be a mode of stating fresh facts; finally in its old age it was to be no more than a cumbersome system of nomenclature.

## CHAPTER VIII

### THE USE OF WEIGHT : JOSEPH BLACK, 1754

THE chemical history of the latter part of the eighteenth century has been so thoroughly and so lucidly told by numerous writers—in especial by the late Sir T. E. Thorpe and by Sir W. A. Tilden—and the *Alembic Club Reprints* have made accessible so many of the original researches, that it would be superfluous here to do more than to show how these researches link those already reviewed to the chemistry with which the nineteenth century was to open.

In the half-century which began with 1750 the central chemical theme, as has already been stated, was still one of classification of substances. But the question, "Is it an element or a compound?" was now being put less baldly than before; the direct attack was necessarily laid by for a time, corpuscular ideas were in abeyance, and much of the experimental work consisted in the forging of an armoury of weapons with which, in due course, siege was once more laid—and this time decisively—to the central position. One part of this process, an outcome of the increasing number of chemical practitioners, was the development of the methods of qualitative analysis. Already Boyle had made use of vegetable indicators for acids and alkalis; he knew and used our tests with silver salts to detect chlorides and calcium salts for sulphates, and likewise he recognized ammonia by its fumes with volatile acids, and used it in turn as a reagent for copper. Numerous other such tests were gradually introduced; and by 1760 Marggraf in Germany had brought in, among other analytical methods, the use of flame-colours, and the application of the microscope in identifying substances by their crystalline form. Undoubtedly the mineralogical chemists, notably Bergman and Scheele in Sweden, made the chief advances in this branch of practical chemistry, and by using their methods they made important additions of new substances to the existing lists of acids, alkaline earths, earths, salts, and elements. So also more precision was reached, as tests became more numerous



and more certain, as to the broad relations between such classes ; for instance, the term " salt " ceased to connote merely anything soluble in water and possessed of a salty taste, and it was recognized that salts are compounds that may or may not be soluble. But these tests and methods, though they were in a sense independent of any theory, could not have been so rapidly developed, nor could they have lent themselves as they did to the purpose of the still more fundamental researches, without the thorough clearing-up on the subject of salts, acids, and alkalis which had taken place by about 1750-1760 at the hands of Joseph Black. Nor could those same fundamental researches have been made until a class of substances of quite a new kind—the class of gases—had been revealed by the work of Joseph Priestley.

With this in view, we must confine our present survey mainly to the effects of the work of Black on the one hand, of Priestley and Cavendish on the other, leading up to the crucial researches of Cavendish and finally of Lavoisier.

Just over a hundred years after Boyle's birth, and within a year of the death of Newton, Joseph Black was born at Bordeaux. His father was a Belfast man of Scotch extraction, and had settled at Bordeaux as an exporter of wine ; and his mother was an Aberdonian. Joseph was sent to school in Belfast, and then to Glasgow University, where he took a course in Arts and then studied medicine. The lectures on chemistry were given by Cullen, who invited Black to assist him while pursuing, at the same time, his professional course. To complete his medical studies, Black then went to Edinburgh, where between 1752 and 1754 he did the great work on alkalis which he presented as his M.D. thesis. Cullen then came to a chair in Edinburgh ; and Black crossed over to succeed him in Glasgow as Professor of Medicine and Lecturer in Chemistry. But he did not specially love the medical part of his teaching ; and when in 1766 Cullen died, Black gladly returned to Edinburgh, to succeed him once more, but now as " Professor of

Chemistry and Medicine." His claims to this post were even stronger than when he had last left Edinburgh; for while at Glasgow he had made his second epoch-making research, into Latent Heat. He retained the Edinburgh professorship for thirty years more, until his death; and the handsome old chair which he used then still serves the professor of chemistry.

Black must in many ways have resembled Boyle. Like Boyle, he remained unmarried, yet he was very far from being unsociable, being fond of friendly company and seeking companionship no less than he was himself sought. In his talk was no trace of affectation or condescension, and he had a store of connoisseurship in music and artistic matters generally which made him *persona grata* in the unusually gifted society of the Edinburgh of his day. Any sort of humbug or effusiveness was distasteful to him, and his native directness could overcome the mildness and charity of his conversation if offence were given him in that way. Throughout his life he was—again like Boyle—very delicate in health; and the quiet regularity of life which this imposed accentuated the sedate and placid habit which was in any case his nature. It was this which made him unable for anything energetic, especially after middle age, so that he did no significant experimental work after his two great researches. In these, as in his lectures, his method was like himself—easy, straightforward, logical, and without either haste or artificial ornament.

Black's thesis on Magnesia Alba had the effect, in the first instance, of showing conclusively the relationships of the mild and caustic alkalis; and a digression upon this is given, in order to illustrate methods which had a still wider issue. It was known that chalk, on being ignited, formed quicklime, which with water slaked to form a caustic alkali; it was known that vegetable ashes (potashes) contained a soluble and mildly alkaline substance, and that soda was also a mildly alkaline body; further, that treatment of either of these two mild alkalis with slaked lime converted them into caustic potash or

caustic soda, precipitating the lime. And it was known that the mild alkalis shared with chalk the property of effervescing when treated with acids.<sup>1</sup>

The only theory which had held the field had been that put forward about 1670 by Dr. Willis on the basis of suggestions made by Bacon : it was a sort of phlogistic theory, for, according to it, chalk in the kiln becomes impregnated with fire-corpuscles, which made it quick and caustic. The heat liberated on slaking this with water was supposed to be due to the release of a part of the fiery matter ; and this in the face of the fact, enquired into by Bacon and confirmed by Mayow, that oils cannot slake lime, though they have an affinity to fire. The theory went on to explain that the fire-stuff remaining in slaked lime could be yielded up to any mild alkali, upon which it conferred the property of causticity hitherto resident in the lime. Thus, the actual facts known were :—

Chalk + heat	—————→	Quicklime
Quicklime + water	—————→	Slaked lime + some heat
Mild alkali + slaked lime	—————→	Caustic alkali + mild lime

And the explanation was :—

Quicklime	=	chalk + much fire-stuff
Slaked lime	=	chalk + some fire-stuff
Caustic alkalis	=	mild alkalis + some fire-stuff

Black first showed that *magnesia alba*, a substance of medicinal virtue, is a mild alkali rather like chalk, not soluble in water, and dissolving in acids with effervescence to yield salts. Like the mild alkalis potash and soda, it acts on slaked lime solution so as to precipitate all the lime in the mild form of chalk, but unlike them it leaves no caustic alkali in solution in the liquid on doing so. Perhaps, however, this new mild alkali, *magnesia*, can be made caustic by heat, as chalk can ? One ounce of *magnesia alba* is weighed out on the scales which

<sup>1</sup> Note that "effervescence" means literally "boiling up"; the term was applied merely to the commotion, without a very clear notion that the release of a gas was involved. Cf. page 74, *note 2*.



Dr. Black, as a physician, no doubt requires for making up his prescriptions; and it is heated red-hot in a crucible for an hour, and is then weighed again before being examined. It has lost quite  $\frac{7}{12}$  of its weight. Nevertheless, when tested with acids, it dissolves to yield precisely the same kind of salts as did the original *magnesia alba*; "but what is particularly to be remarked, it is dissolved without any the least degree of effervescence." And—an important point—contact with water does not give back to ignited *magnesia* the power of effervescing with acids, so that it was probably not merely water that it had lost during the ignition. Moreover, it differs from the original in having lost the power to precipitate lime-water.

What *had* been lost on ignition? Some *magnesia alba*, of course carefully weighed out, is heated in a retort fitted with a condenser. There is a large loss of weight, as before, but of the total loss less than half is accounted for by a quantity of water collected by the condenser; which confirms the view that water was not the main material to be expelled. But "Chemists have often observed, in their distillations, that part of a body has vanished from their senses, notwithstanding the utmost care to retain it; and they have always found, upon further enquiry, that subtle part to be air, which having been imprisoned in the body, under a solid form, was set free and rendered fluid and elastic by the fire. We may therefore safely conclude that the volatile matter, lost in the calcination of *magnesia*, is mostly air; and hence the calcined *magnesia* does not emit air, or make an effervescence, with acids."

This being so, asks Black, where did the air in *magnesia alba* come from? Dr. Hales' experiments are enough to tell us that if there are any vegetable impurities in the body, they will yield "air" on heating; so their initial absence must be ensured. So this time, a weighed quantity of *magnesia alba* is strongly ignited, to destroy vegetable impurities; on being re-weighed, it has lost weight; it is then dissolved in acid and re-precipitated with mild alkali, and the precipitate, when collected and dried, has practically the original weight and

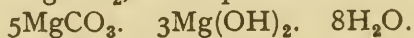
answers *all* the tests of *magnesia alba*. Thus the ignited *magnesia* has, by treatment with mild alkali, not only regained the weight it had lost in ignition, but has recovered also the air which makes it effervesce; consequently this air can have come only from the mild alkali applied to it.

The next step is to see how much air was lost in effervescence. Careful measurements are made: to a weighed flask, containing an ounce of water and a weighed quantity of material, acid is added until it ceases to be neutralized, when the acid required and the whole vessel and contents are weighed. The results show that something substantial has escaped from the flask during the reaction, excepting where *ignited* *magnesia* had been used.<sup>1</sup>

The quantity of acid required for neutralizing *magnesia* is the same whether the *magnesia* has been ignited or not before the acid is applied.

Reflecting on these results, Black saw that his "air" is something which played the part of an acid; of feeble activity, set free from *magnesia alba* by ignition, or displaced from it, with effervescence, by other acids; and that it is also present in mild alkalis. But, if this were so for *magnesia*, must it not be so similarly for chalk? And in that case, is the "remarkable acrimony" of ignited chalk rather a property of the quicklime itself than due to the access of fiery matter; and is the mildness of chalk due simply to the union of quicklime with the acidic "air"? And is not the slaking of quicklime simply its chemical union with water, whose place can alternatively be taken by the acidic air? Here was a set of novel propositions, which entailed a train of further experiments on lime and the alkalis. We have seen enough of Black's method to let us believe that the rest of the research was carried out as clearly and conclusively as the other;<sup>2</sup> we need only remark that the

<sup>1</sup> Cf. Boyle's experiments described on page 73. Black's data, if calculated on a modern basis without regard to errors inherent in the method of determining  $\text{CO}_2$ , are equivalent to the formula



<sup>2</sup> See Alembic Club Reprint No. 1.

whole inference was completely substantiated, and the relationship of the fixed or mild alkalis to caustic alkalis was quantitatively cleared up once and for all.

Finally, there was the nature of the "air," which had been weighed but not isolated. Black found that since lime-water remains unclouded in a stoppered bottle containing air, "Quicklime . . . does not attract air when in its [air's] most ordinary form, but is capable of being joined to one particular species only, which is dispersed thro' the atmosphere, either in the shape of an exceedingly subtile powder, or more probably in that of an elastic fluid. To this I have given the name of fixed air. . . ." Before long,<sup>1</sup> Black added in his lectures the further information that this fixed air is the same as the suffocating air of mines and grottoes, the same as is emitted by fermenting vegetable matter, and that it also agrees in some respects with air which has been vitiated by the breathing of animals or by the burning of fuels; and that it is not the same air as is yielded by the dissolution of metals in acids. It remained for Lavoisier, later on, to make the demonstration—crucial for organic chemistry—that the gas is the product of combustion of carbon.

Quite apart from the special class of substances investigated by Black, the method which he applied is of the highest significance. Others before him, such as Boyle and a little later Homberg, had used the balance for studying chemical reactions; but whenever a gaseous substance had, unknown to them, entered into the action as a reagent or as a product, they had been defeated. Black, on the other hand, trusted first and foremost to his balance and to the principle of the Conservation of Mass, making search for chemical participants in the reaction only after the balance had showed him that they were there to be sought. His notable openness to the instruction of the balance must have been very greatly helped by the fact that

<sup>1</sup> *I.e.*, for several years before 1773; see Sir J. Pringle's Discourse at the Anniversary meeting of the Royal Society of that year.



he happened to begin this research with a substance of completely unknown nature, associated with no existing theory whatever, and he was thus forced back upon first principles. But for the success with which they enabled him to solve the totally new problem of *magnesia alba*, there is no likelihood that Black would have thought of applying these principles to open up the old problem of chalk and lime.

In logical strictness, there was now nothing lacking which could prevent the real nature of metallic calcination from being discovered, for if we imagine Black to have substituted red oxide of mercury for chalk and mercury for quicklime, we can perceive that his own mode of discovering "fixed air" would have led him to oxygen. This comment is made merely to emphasize the fact that Black's work is the first sign of an all-important change whereby the Conservation of Mass was to become a fundamental basis of chemistry. But we have seen already, from other examples, how large a part habituation plays in the practical adoption of ideas whose abstract truth may be admitted, and so also here; for most people the principle of the Conservation of Mass still required strengthening in its only puzzling aspect—the materiality of gases—and the chemical world still needed the gaseous discoveries of Priestley and Cavendish to make it realize that all the states of matter were within its scope. To these discoveries we are about to turn, in their relation to the unseating of phlogiston and the reconstituting of chemistry.

## CHAPTER IX

### LATER EIGHTEENTH CENTURY: THE REALITY OF GASES

A GENERAL view of the quarter-century from 1765 to 1790 shows it as quite a dramatic period in chemistry, in marked contrast to the barren half-century which went before. First we have the discoveries of Cavendish, published in 1766-7, on "Factitious Airs," in which he opened the way towards the recognition of the individuality of gases, developed quantitative methods for their examination, and brought forth new facts in the reactions between metals and acids, which he used the phlogistic hypothesis to explain. Next we have Priestley, coming forward with a new weapon for experimenting, with which he made the most astonishing discoveries of artificial airs. Off-stage, Scheele—the first great continental chemist—was labouring hard in Sweden and was adding hardly less astonishingly to the knowledge of chemical substances and changes. Not only were all three strong adherents to the phlogistic hypothesis, but their work served to extend it and to bring it into greater prominence than ever before. Meanwhile Lavoisier in Paris, the first to grasp fully the meaning of the quantitative weapon wielded by Black, was being led by his researches to an increasing dissatisfaction with the phlogistic theory, and perceived that a complete inversion of ideas might become necessary. Lavoisier's genius and his service to chemistry lay, however, rather in applying quantitative principles and a fine technique to already-known qualitative facts than in finding new material; and he did not himself obtain the clue of which he was undoubtedly in search. This was left for Priestley and (unknown at the time) for Scheele; for by the ironic workings of Providence, Priestley's crowning discovery was the undoing of his own scientific faith. In oxygen, Lavoisier saw immediately the substance that he was seeking, whose discovery cut at one stroke the knots which phlogiston had tied round chemistry; he applied and made quantitative the experiments of Priestley, and proved all but conclusively

the essential truth of his new claims. As yet, however, he had not come to the full truth; it was still possible for his critics to urge that union with oxygen is accompanied by emission of phlogiston; and in knocking down an idol he had raised its substitute too high by crediting it with too universal powers of acidification embodied in his name for it—oxygen. Once again, then, the action passes to England, where a group of chemists, including Priestley but mainly following Cavendish, were interested in the nature of that most ancient of Elements, water. In 1781–1784, still working by the light of the doomed phlogistic hypothesis, Cavendish by experiments of masterly accuracy performed the synthesis of water and measured its composition, and he proved also the composition of nitric acid. Even so, Cavendish's proof of the compound nature of water was weakened by his expression of it in terms of a modified phlogistic view. Lavoisier on his part, incredulous that so neutral a body as water could be thus formed from his acid-maker, oxygen, yet recognizing no alternative explanation, repeated the synthesis and accepted the facts. And, by asserting unambiguously the compound nature of water and the composition of the oxides of carbon, he was able to proclaim finally his proved theory of combustion, oxidation, and reduction.

If Boyle's *Sceptical Chymist* is for chemistry the Book of Genesis, Lavoisier's *Traité Élémentaire* is its Book of Exodus, for it shows how he led his people out of a captivity that had endured for nearly a century. Most French chemists were converted to the truth; the British soon followed, headed naturally by Black, the father of gravimetric chemistry; by 1800 the Germans and the Swedes had come into line; and the change was over. There remained only the Berthollet-Proust controversy to be settled, regarding the definiteness of chemical compounds; and at last chemistry was able to point to the genuine elements which a real analysis of matter had revealed, and it could proceed to deduce—no longer merely to premise—a demonstrable theory of elementary atoms.



With this survey as our guide, we may see a little of two of the men, and then follow the effects of their work. The first in time and the first in all-round scientific genius was Henry Cavendish.

Cavendish's forbears on his father's side were the Dukes of Devonshire, on his mother's the Dukes of Kent. He was born in 1731, and was sent as a boy to school in Hackney, whence he went to Peterhouse, Cambridge. His younger brother joined him soon after, but both left without proceeding to degrees. In his twenty-first year, Henry Cavendish came to live with his father in London. The father, Lord Charles Cavendish, had been for nearly twenty years a widower, and had in that time given considerable attention to practical physics. He invented two forms of maximum and minimum thermometer, worked on the compressibility of water, and was one of the first after Hooke to study the capillary depression of mercury surfaces in glass tubes; this work gained him the Fellowship of the Royal Society soon after Henry Cavendish came back from Cambridge. If the father had hoped that his son would take a place in London society, he was disappointed, for it was quite impossible to persuade Henry to do any such thing. Cavendish was slightly built, insignificant in feature, and had a shrill voice and hesitating utterance; he paid no attention to his personal appearance, and he shrank from entering any gathering. He had not the faintest interest in any person or thing outside physical science. This was not due to egoism or to arrogance, rather the contrary; he probably looked upon human beings, himself included, as an entirely superfluous creation of extravagant and irrational complexities, planted in the midst of an otherwise orderly universe; and he made it his business, in studying that universe, to avoid, as far as might be, all contact with such disturbing anomalies as his fellow-men. And as to his fellow-women, he seems to have regarded them as an even worse blot on the fair text of nature's laws. It must have been a lasting shame to Cavendish that once on Clapham Common he was so far overtaken by irrational

impulse as to rescue a female from the menaces of a cow; and a single meeting with a busy housemaid as he went upstairs and she came down was enough to make him order a back staircase to be built, to avoid such distressing encounters. Need it be added that Cavendish never married? For nearly thirty years he lived with his father, at first helping with his experiments and then going on with his own. He was elected F.R.S. in the first year of George III's reign, and continued his great researches during the next forty years. When he was about fifty, Cavendish inherited very great wealth. He acquired two or three houses in London, one of which, in Soho, he converted into a scientific library; another is at the corner of Gower Street and Montague Place. This has been marked by a commemorative tablet—ornamented with little Cupids. Later in Cavendish's life he bought a house at Clapham, making the drawing-room into a laboratory, the boudoir into a furnace-room, the top floor into an observatory, and a fine tree in the garden into a meteorological observation-post. The recording angel of Clapham, if there is such a being, must have been sorely puzzled to discover the conditions of eligibility to the Royal Society; for the other Fellow who had elected to end his days there had been Samuel Pepys.

It may be allowable to pity Cavendish for his non-human ways; but as a scientific machine served by eyes and hands, he is almost unapproached, and the debt which physics and chemistry owe to him is enormous. His work in electricity, magnetism, heat, and energy, gravitation, astronomy, meteorology, is classical; in much more which he never published he practically discovered Dalton's law of partial gaseous pressures, Charles' law of gaseous thermal expansions, studied the effusion of gases, discovered the Joule-Thomson effect, discovered, independently of Joseph Black, latent and specific heats; in pure chemistry, outside his publicly recorded work, we find that he had a remarkable knowledge of analytical work, involving the law of combination in definite proportions, while

also he anticipated some of Scheele's work on chlorine and on tartrates.<sup>1</sup>

Whatever he studied, he measured; quantity was his only passion, and even theory he preferred to leave to others.

Joseph Priestley offers a rather obvious contrast with Cavendish. He was born near Leeds in 1733—five years after Black, two years after Cavendish. His father was a cloth-worker, and poor; so, his mother having died when he was young, he was brought up by a kind, if strictly-minded, aunt, and he was trained as a Nonconformist minister. He gained much mixed learning, which he made use of when, his first ministrations having proved unacceptable to his flock, he went to take up teaching at the Warrington Academy. Here, in the early 'sixties, he married the daughter of an ironmaster, named Wilkinson; and one of their friends was Benjamin Franklin. This friendship turned Priestley to electrical experiments, at which he soon made such a name for himself as to be made F.R.S., and indeed, very nearly to be awarded the Copley Medal. He and his wife returned to Leeds; and about 1770 he began to take up, in the intervals of his chapel work, the study of gases, this being suggested by the large amount of "fixed air" available at a neighbouring brewery. In 1772, however, the family moved again, this time to a post which brought much more security and leisure for the hobby which was filling quite half of Priestley's mind. Half only, because with all his scientific research, Priestley was an ardent theologian who published voluminously on all manner of religious topics. This new post was that of librarian or literary companion to the Earl of Shelburne, 1st Marquis of Lansdowne, at Bowood in Wiltshire. It will be remembered that Priestley's patron, to whose bounty to him chemistry owes so much, was the great-grandson of William Petty of the Invisible College.

<sup>1</sup> See Cavendish's *Collected Researches*, published by the Cambridge University Press, the volume on chemistry being prefaced with an introduction by Sir T. E. Thorpe. Also George Wilson's *Life of Cavendish*, 1851.



After seven fruitful years, Priestley's theological polemics made it no longer possible for Lord Shelburne to keep him at Bowood, though he generously presented him with an annuity; and in 1780 Priestley was appointed to a new charge, at Birmingham. Here for eleven years he continued his scientific and clerical labours; but he had associated himself with the revolutionary ideas which became rife at this time of the French upheaval, and in a mob-protest against these doctrines his house and property were destroyed. Coming to London, he suffered still from his visionary leanings; his peculiar convictions, which he never ceased to propagate, were not of the kind which could safely be tolerated in those unstable days, and authority justly looked askance at anything likely to weigh on the wrong pan of the delicately-poised political balance of 1793. So Priestley followed his sons to America, where he lived peacefully, but still vigorously writing, until his death in 1804.

Priestley was a keen debater, simple in mind, unconsciously dogmatic, and receptive only up to a fairly well-defined limit. He held fast to the phlogistic faith in which he had trained himself, and defended it no less stoutly than he did his political and religious nonconformity. With all his doggedness in argument, he was easy and open in his science, and was roused to vehemence only when his generosity was abused by misrepresentation. The openness of his mind was limited partly through lack of knowledge, partly through lack of logic; but so explicit is he in his writings that the honesty of his purpose and of his train of thought is clear on every page. Let us read an excerpt from one of his prefaces :—

“ I even think that I may flatter myself, if it be any flattery, as to say that there is not, in the whole compass of philosophical writing, a history of experiments so ingenuous as mine; and especially of the Section on the discovery of dephlogisticated air, which I will venture to exhibit as a model of its kind. I am not conscious of

having concealed the least hint that was suggested to me by any person whatever, any kind of assistance that has been given me, or any views or hypotheses by which the experiments were directed, whether they were verified by the result, or not. . . . In the mean time, the field is as open to others as to myself."

His use of the word "ingenuous" is certainly correct; he was so honestly charmed with the results of his own work, and so pleased at the encouragement of others, that often we cannot help a friendly smile at his remarks. For example, when Priestley received the Copley Medal of the Royal Society in 1773, the President printed an interesting eulogy of Priestley's work; and in a subsequent publication Priestley says, "See Sir John Pringle's Discourse . . . which, if it became me to do it, I would recommend to the reader." Again, his simplicity and charming optimism are shown when, having described an experiment which overthrew the opinions of a certain adversary, one Alexander, he writes, "As Dr. Alexander appears to be an ingenious and benevolent man, I doubt not but that he will thank me for it."

One could almost wish that Priestley's work had stopped when he had discovered oxygen. Lavoisier required no more from him to build his new structure, but in so doing he demolished all that Priestley held dear in chemical theory. And Priestley, faced with his own discovery—the very negation of phlogiston—had recourse to strange imaginings. His circuitous and blurred modifications of the ordinary phlogistic hypothesis seemed to him to be perfectly plausible, and he reproaches himself for having not earlier perceived them, in words which his opponents might well have quoted against him as his own scientific epitaph:—

"We may take a maxim so strongly for granted, that the plainest evidence of the sense will not entirely change, and often hardly modify our persuasions; and the more

ingenious a man is, the more effectually he is entangled in his own errors; his ingenuity only helping him to deceive himself, by evading the force of truth."

It is sometimes said that Priestley was no more than a dilettante scientist; but this is most unjust. His reasoning, it is true, was often at fault, and he did not always distinguish the accidental from the essential; but he continually worked from one group of experiments to another by inference and hypothesis. He played for high stakes; like Ramsay in our time, he preferred to throw his line for a few rare salmon rather than for a basketful of troutlings; and we may close our sketch of him with words which were applied in his own day to a much earlier chemist:

"It is just possible to conceive that a single important discovery may be made without much sagacity; an undiscerning eye may perhaps be so placed as to be aware of the manner in which nature performs some one of her hidden operations, but he who shall surprise her often, must be allowed to have the discerning eye, and to know where the proper point of view is to be found."

In order to grasp the enlargement of the field of chemistry which Cavendish's earlier researches and those of Priestley brought to pass, it is well to realize the general attitude towards gases prior to this period. The only familiar gas, that of the atmosphere, showed extremely wide variations when it was diagnosed by the most obvious and natural test—that of smell; and this was undoubtedly even truer in the seventeenth and eighteenth centuries than it is now. Besides this, it feels sometimes damp or dry, choking or bland; yet nobody would call it anything but air, in all these conditions. No one could know that such apparently profound differences were really due to trace-impurities, however the invasion of air by accidental fumes might be admitted; and the only natural assumption, closely linked with the ancient doctrine that Air is one of the



four Elements, was that air is capable of putting on very various qualities at different times. When, therefore, a parcel of "air" made artificially was (for example) seen to be brown, or to have a rather more striking smell or pungency than had hitherto been remarked even in seventeenth-century town air, it was simple enough to attribute the differences between this parcel and atmospheric air to no more than the same variability, carried perhaps a little further. So "air" might vary largely, as well as being capable of "infection by roving steams." And the early tests confirmed this rather than otherwise; for instance when Mayow, like Boyle, prepared hydrogen, he found it and air to be equally compressible, so that they were the same in this respect, though one was and the other was not fit for breathing. Van Helmont, and later Boyle and Hooke, had some suspicion that individual gases exist, but no more; and doubtless other chemists here and there had inklings of the fact, but none of them particularly attended to it. The Rev. Stephen Hales, who published his *Vegetable Staticks* in 1727, did a great deal to illustrate the release of gas from solids and liquids by chemical change; but although he prepared several different sorts, including chlorine, nitric oxide, hydrogen, carbonic acid gas, and oxygen, all his gases were reported as "air," variously impregnated so as to be endued now with one property, now with another. Later, Dr. Brownrigg, of Whitehaven in Cumberland, found the mephitic exhalations or choke-damp of coal mines to be "a fluid permanently elastic"; but probably the first definite assertion of separate gaseous entities was that of Black, already quoted, concerning "fixed air," in 1754. Thus we arrive at the period of the earlier of Cavendish's two groups of chemical researches, published in the *Philosophical Transactions* of 1766 in three papers under the title "Experiments on Factitious Air."

At present only those parts of the work which bear on the general existence of diverse gases need be cited. The first section deals with what we know as hydrogen, up to that time identified solely by reference to its being inflammable. Caven-

dish shows that "inflammable air" is produced when either tin, zinc, or iron is treated either with spirit of salt (hydrochloric acid) or with diluted vitriolic (sulphuric) acid. For the moment we shall defer Cavendish's correlation of this gas with phlogiston, noting simply that he regarded the inflammable air as having emerged from the metals used. It could be stored in bottles inverted in water, and was not absorbed by the water; nor was it affected by alkalis. Its already-known property of exploding was studied by igniting mixtures of it with air in various proportions; an 'optimum' ratio was found, and it was found that mixtures too rich in either constituent would only burn but not detonate; neither would burn, of course, in the absence of the other. (No notice was taken of the products of the combustion.) Using the loudness of the explosion, under standard conditions, as a guide, Cavendish found no difference between samples of inflammable air, from whichever metal or acid they had been made.

The next step, and a significant one, was that the specific gravity of the gas was determined. At first, a bladder was used in which to weigh it: just as Francis Bacon had done with the vapour of alcohol, but needless to say Cavendish did it much more skilfully and with better appliances; moreover he made the comparison with the weight of the same volume of air. Thereafter he changed the method; having first found the volume of gas yielded with a given weight of zinc, tin, or iron,<sup>1</sup> he made separate measurements of the loss of weight sustained by a vessel in which a known weight of one of these metals was allowed to dissolve in acid so that the gas escaped; the issuing gas was prevented from carrying off moisture, by the provision of a drying-tube. By these methods Cavendish found his inflammable air to have one-eleventh the density of atmospheric air: a difference which could not fail to emphasize the deep-seated difference between the two.

<sup>1</sup> These volumes, corrected for the temperatures and pressures duly recorded by Cavendish, are in near accord with modern determinations.

The second section deals with Black's fixed air, which name Cavendish retained. The gas was found to be soluble in water, unlike common air or inflammable air; and measurements were made of this, with the aid of mercury as the confining fluid—so used for the first time. Owing probably to accidental contamination with ordinary air, fixed air was found to contain a less soluble part; which led Cavendish to an opinion certainly erroneous in this instance, but significant from our present standpoint, that "fixed air from marble consists of substances of different natures." The solubility in spirit of wine and in olive oil was also measured. Next, the specific gravity of the gas was determined, with a bladder, and was found to be 1.57 times that of air. A further set of systematic trials was made upon the power of fixed air to suppress the flame of a candle, the duration of the flame being carefully correlated with the proportion of fixed air admixed with the air of the vessel; and the remarkable potency of a small admixture in extinguishing flame was noted. Lastly, a set of measurements was made to weigh the quantity of fixed air contained in combination in various carbonates.

In the third section Cavendish treats of the airs produced by fermenting and putrefying material. By using the tests of absorption in alkali and in water, the suppression of flame, and specific gravity, he confirmed the production of fixed air in fermentation. The material exhaled from putrefying matter into the air of the receiving vessel was treated with alkali to absorb fixed air from it, and left some inflammable air mixed with the already-present atmospheric air; the ratio of these was estimated, but the specific gravity of the mixture was found to be more than that of a similar mixture made of air and hydrogen. It was inferred that the inflammable air from putrefaction is "nearly of the same kind as that produced from metals," but "not exactly the same, or else to be mixed with some air heavier than it."

From these particulars it will be clear that Cavendish, at least, had no qualms whatever in admitting "airs" to the



category of chemical substances, and that he plainly recognized the existence of several diverse species, distinguished by having different densities as well as perfectly definite and different chemical properties. And this was the knowledge with which Priestley began making his researches in 1770, certain results of which may be noted.

Starting, as we have seen, with the study of fixed air, Priestley incidentally invented soda-water, and wrote a pamphlet of instructions on it, for the benefit of the Navy. He developed a simple technique with the "pneumatic trough," and with mercury as the confining fluid, imitated respectively from Brownrigg and from Cavendish; and it was the faithful use of such apparatus, comparatively simple and easy to apply to numerous materials, that enabled Priestley to make a great wealth of experiments. Among them are here to be noticed those on inflammable air; he made samples by Cavendish's way from metals and acids, also by heating organic matter in a gun-barrel; but he took it that both were the same kind, as they smelt the same; and there was a good deal of contamination with ordinary air which misled him. Before 1772 he prepared nitrous air (nitric oxide) from metals and nitric acid, and found that it acted on common air to yield a brown fume, with an accompanying diminution in the volume. Discovering that this effect of nitric oxide went hand in hand with the fitness of air for respiration, he says "from this time, I had no occasion for so large a flock of mice"; and henceforth the "goodness" of any air was always examined by measurement of the volume-change which ensued upon admixture with nitrous air in the presence of water. By exposing nitrous air to iron nails over mercury, a new air was obtained in which a candle would burn very well, but which was quite noxious to animals, and was also easily absorbed by water; this was evidently nitrous oxide, laughing-gas. The action of pure charcoal heated in air over mercury with a burning glass yielded inflammable air—actually carbon monoxide, unrecognized.

Cavendish had found a curious air to be produced when he

heated copper with marine acid; Priestley now found that no copper was necessary, and with the aid of his mercury-trough he showed that "this remarkable kind of air is, in fact, nothing more than the vapour, or fumes of spirit of salt . . . of such a nature that they cannot be condensed by cold, like the vapour of water and other fluids, and therefore may be very properly called an *acid air*, or more restrictively the *marine acid air*." In showing that this was the active constituent of marine acid, the rest being "mere phlegm or water," Priestley probably helped towards the general ignoring of water as an essential constituent of most other acids, which was not put right until the next century.

Following up these facts, Priestley isolated gaseous ammonia, the actual 'volatile alkali'; he examined it, and found that by passing electric sparks through it, the volume increased, it became insoluble in water, and produced inflammable air.

The other gases isolated at Bowood included sulphur dioxide, ethylene, and silicon fluoride; he knew little of their real composition, but the mere fact of being able to handle and test them was enough. The discovery of oxygen in 1774 has been too often told to need more than a reference to the Alembic Club Reprint (No. 7) of the original account.

These, then, were the facts which, broadcast by the wide demand for Priestley's publications, drove home the reality of gaseous species; and only two final steps in this process call for mention. In 1781 Cavendish standardized Priestley's mode of testing the goodness of air, and was able to prove that during sixty days there were no measurable changes in the composition of the air of London. After all that had been believed of the great fluctuations in the air, "our sense of smelling can, in many cases, perceive infinitely smaller alterations in the purity than can be perceived by the nitrous test." Parenthetically, here is rather a pretty point as to the reversal of an interpretation of a given set of facts, which comes from the accumulated experience of other facts; for Cavendish and we with him have learnt to distrust the evidence of the obvious

and qualitative sense of smell, whereas if this single discovery of his could have been isolated and taught to philosophers in, say, the fourteenth century, it could have been legitimately held to exemplify perfectly the ancient doctrine of independent Qualities explained in Chapter V.

The other remaining mark, and the last abolition of all puzzlement as to elastic fluids, was made when Lavoisier, in reorganizing chemistry, adopted the forgotten name "Gas" as a generic term, and so dispelled finally the last shadow of the Aristotelian notion that Air is the basis of all elastic fluids.



## CHAPTER X

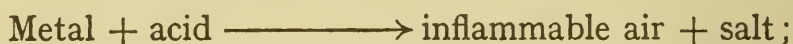
### THE LAST OF THE "FOUR ELEMENTS"

It remains now to review, as briefly as possible, the last phase of the phlogistic hypothesis. It was remarked in an earlier chapter that this, a hypothesis of combustion, was found in the long run to explain almost everything in chemistry except combustion. Chemists need no reminding that, apart from the combustion of metals and of fuels, phlogiston ought to have accounted for every instance of oxidation and of de-oxidation; and it is a testimony to the perspicacity of its advocates that it actually did so. One of the earliest examples of this was apparently due to Stahl himself, who explained the production of red vapours from the treatment of nitric acid with combustibles by saying that the vapours were nitric acid united with phlogiston from the metals. The same was held to be true of the conversion of sulphuric acid to sulphurous acid by the action of combustible bodies; and Cavendish in 1766, Scheele in 1774, Priestley, and in short all chemists, conceived of what we call an intermediate stage of oxidation as being an intermediate stage of dephlogistication. The presence of phlogiston is equivalent (qualitatively) to the deficiency of oxygen; sulphuric acid, sulphurous acid, and sulphur respectively represent to us a complex compound of oxygen, a less complex compound with less oxygen, and an element, united with no oxygen at all; to the phlogisticians, sulphuric acid was the simple body, sulphurous its first compound with phlogiston, sulphur its most complex compound, having in it the most phlogiston.

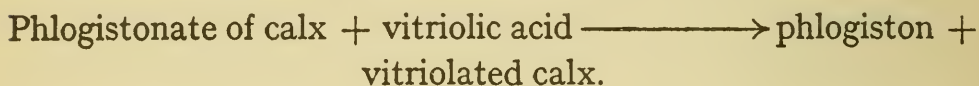
Similarly, when in 1774 Scheele discovered chlorine by oxidizing hydrogen chloride with manganese dioxide, he thought he was causing phlogiston to quit the marine acid (hydrogen chloride) in order to attach itself to the manganese, so leaving "dephlogisticated marine acid."

It will be seen from the last example that the function ascribed to phlogiston was sometimes not far from the part which is in fact played by hydrogen; just as, for us, the term

"reduction" can imply a removal of oxygen or an addition of hydrogen. And the identification of phlogiston with hydrogen had in truth already been made. In Cavendish's paper on Factitious Air, already alluded to, he had performed reactions which can be described as :—



and his interpretation of this result can be expressed as, *e.g.* :—



The evolution of hydrogen from acids and metals was thus put precisely on a par with Black's correct conclusion regarding the evolution of fixed air from acids and mild alkalis (carbonates): just as chalk is a dissociable compound of lime with fixed air, so was a metal taken to be a dissociable compound of its calx with inflammable air or phlogiston.

It is profitable to notice that Cavendish's identification of the material gas hydrogen with the hitherto intangible phlogiston, so far as it was meant as an assertion, depended on a very simple fallacy in logic. The argument took some such course as: '(i) Others have said, on sundry grounds, that all metals are compounds containing phlogiston; (ii) here is something new that has come forth out of a mixture of either one of two acids with any one of three metals; (iii) therefore it is probably phlogiston.' The fallacy lies, of course, in the decision to neglect the chance that the two acids were sources of the inflammable air; a decision which Cavendish buttressed with a further fallacy in logic by urging that the air was unlikely to have come out of the acids, because it could be generated also from materials which were not acids. (Neither were they metals.) Yet the conclusion, although it was only a possibility and not a necessity, remained unchallenged either in England or France for nearly twenty years by everyone excepting Lavoisier only, who was assailing, not the conclusion, but the initial premiss (i) above; and it suffered only one proper

modification during that period, at the hands of its originator himself. This modification arose from his epoch-making work during which he synthesized water; and it is necessary to dwell a little on some salient points of that work.

The researches in question were published in 1784,<sup>1</sup> and their object was to find out what makes the volume of a quantity of air grow less when things burn in it, and what becomes of the air thus lost. It may be recalled that oxygen had been discovered ten years before by Priestley and by Scheele, and that the latter's great chemical researches on Air and Fire <sup>2</sup> had been published for seven years: also that Lavoisier had already put forth much of the evidence for the anti-phlogistic theory. Nevertheless, Cavendish's treatment of his theme is very nearly self-contained, though partly suggested by observations made by others, and it rests on almost no other experiments than his own, together with a knowledge of how to prepare his materials.

That air should diminish when substances poured their phlogiston into it was naturally puzzling; and Priestley, who had devoted much attention to the fact, approached the only explanation which modern chemists might consent to reconcile with other facts of gaseous combination,<sup>3</sup> when he said "In what *manner* air is diminished by phlogistication independent of the precipitation of its constituent parts, is not easy to conceive; unless air thus diminished be heavier than air not diminished" which, however, he did not find to be the case; nor did Scheele, who raised the same question. Cavendish's work can be appreciated at all properly only by reading his own account of it, and no chemist would fail to do this; but attention here is drawn to that part of it in which he used the combustion of hydrogen as one of the means of phlogisticating air.

<sup>1</sup> Alembic Club Reprint No. 3.

<sup>2</sup> The chief of which are to be read in Alembic Club Reprint No. 8.

<sup>3</sup> Namely, that gases can in fact combine so as to undergo a large decrease in volume; in this case "phlogistonate of air" would be the product, and it would be denser than air because of having greater mass than the air alone and because of occupying less volume than it.



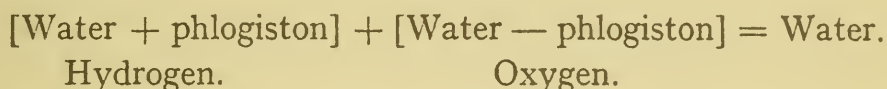
Using his own unrivalled technique for analyzing gases, he made a systematic series of combustions with varying proportions of air and hydrogen, concluding that when they are mixed in the ratio of 100 to 423 and exploded, "almost all the inflammable air, and about one-fifth part of the common air, lose their elasticity and are condensed into the dew which lines the glass." And when he arranged to collect enough of the dew to be examined thoroughly, he found it to be pure water. Consequently, under the conditions just mentioned, the inflammable air and about one-fifth of the common air "are turned into pure water." Cavendish then replaced common air by Priestley's dephlogisticated air (*i.e.*, oxygen) for the explosions with hydrogen; and here he was deflected into a subordinate research which established the fixation of nitrogen and its combustion to form nitric acid. Now returning to the triple question of the nature of pure oxygen, the nature of the water which was the sole product of its union with inflammable air, and the nature of inflammable air, he gives his most favoured interpretation of the experiments as follows:—

" . . . I think, we must allow that dephlogisticated air is in reality nothing but dephlogisticated water, or water deprived of its phlogiston; or, in other words, that water consists of dephlogisticated air united to phlogiston; and that inflammable air is either pure phlogiston, as Dr. Priestley and Mr. Kirwan suppose,<sup>1</sup> or else water united to phlogiston; since, according to this supposition, these two substances united together form pure water."

This conclusion sounds almost a rigmarole; and it has purposely been quoted here because it illustrates perfectly how the phlogistic hypothesis had outlived its usefulness, since it had actually entangled a great and a clear-headed investigator in this maze; and it exemplifies admirably Francis Bacon's *Idols of the Theatre*—"words stand in the way and resist the change." Actually, Cavendish did acknowledge that "dephlo-

<sup>1</sup> And as Cavendish himself had originally suggested—*ante*, page 132.

gisticated air " and " phlogisticated air " were really names for two truly distinct substances (oxygen and nitrogen), and that the two of them mixed together are what form air, as Scheele and Lavoisier claimed. But he does not abandon phlogiston itself, and he says that hydrogen gas is either simple phlogiston, or else, more probably now, a compound of water with phlogiston. (For had it been bare phlogiston it would, he thinks, have united instantly with dephlogisticated air without requiring ignition.) His own synthesis of water, on the latter view, might be expressed as follows, though at the risk of taking his phraseology too literally :—



But it is all too perilously near to circular statement, like several other attempts to make compromises between phlogiston and oxygen ; and there are two flaws which are much more than mere words. For in the first place his retention of phlogiston, with his association of it with hydrogen and his statement that the active part of air is the same as water deprived of phlogiston, imply that in every aërial combustion, water must be one product. This was an implication that had not been established. In the second place, the gravimetric facts known to Boyle, and the gravimetric proofs adduced by Lavoisier, are not discussed. In an interesting comparison drawn by Cavendish in a postscript to his paper between Lavoisier's theories and his own, he says " . . . not only the foregoing experiments, but most other phænomena of nature, seem explicable as well, or nearly as well, upon this [Lavoisier's] as upon the commonly believed principle of phlogiston . . . " ; and after further exposition, he says " . . . as there are, perhaps, no bodies entirely destitute of water, and as I know of no way in which phlogiston can be transferred from one body to another. without leaving it uncertain whether water is not at the same time transferred, it will be very difficult to determine by experiment which of these opinions is the truest ;

but as the commonly received principle of phlogiston explains all phænomena, at least as well as Mr. Lavoisier's, I have adhered to that."

There Cavendish left it, to go on with his experiments; and when this great quantitative experimenter did not own the compulsion of the exact proofs of Lavoisier, it is not to be wondered at that the majority of chemists were encouraged to hold out for yet a few years.

Thus stimulated at the last to spasmodic efforts which only exposed its innate weakness, the phlogistic theory died fighting; its downfall was all the more conclusive, and the supremacy of its successor was all the more completely secured. In those magnificent researches of Lavoisier which are familiar to every student, he showed himself as the first who applied unflinchingly the principles of the Conservation of Matter and Mass to the direct study of combustion and calcination; and by weighing *all* the participants, including those which are gaseous, he demonstrated that not only does one constituent of air unite with the fuel, but further that the other constituent of air plays a purely passive part, suffering no combination with anything emitted by the fuel. The union with atmospheric air which had been discovered so long ago, and which phlogistians had admitted to occur as a merely secondary event ensuing upon an efflux or transfer of phlogiston, was now proved to be the primary event; the union was further narrowed down to one widely distributed simple substance, oxygen, and the relic of the Element of Fire was at last recognized as an illusion. With it had vanished the Elements of Water and Air and Earth, so that the ancient stronghold invested by Boyle was finally carried by assault by Lavoisier.



## PART 4

### THE SEARCH FOR THE STRUCTURAL UNITS



## CHAPTER XI

### THE PROBLEM, 1800-1925

THE record of achievement in chemistry from Boyle to Lavoisier is due to the pursuit of a patient analysis of matter, the search for its ultimate elements being rewarded by the discovery that, in practice, nature stopped short of permitting the analysis to reveal the uttermost simplicity, but exhibited instead a fairly numerous collection of diverse elements of which terrestrial matter is composed. The only instruments of resolution available were heat and the attack of one substance by another : in short, chemical agents, in the narrowest sense of the term. There was soon to be added the agency of the electric current, but even that failed to expose any substances of a character more primitive than the simplest already revealed ; and not until near the outset of the twentieth century were any methods found which could effect an analysis more drastic than those of the older chemistry, to disclose the still more elementary constituents of the chemical elements themselves.

There was nevertheless one outcome of the attention paid by chemists to physics, which must be singled out as a supreme discovery. This was, that our terrestrial elements are universal : that in the sun and in the stars, the spectroscope can detect and does detect elements identical with those of the laboratory. The scope of this discovery is no less than that of the universality of gravitation, and it is indirectly due to Isaac Newton, as the other is directly due to him. The chain of workers upon Newton's spectrum is a long one, but it is to Bunsen and Kirchhoff in 1860 that we chiefly owe the elevation of chemical elements from terrestrial into cosmic. The work is still going on, more actively than ever, and, with the indispensable conjunction of modern mathematical physics, laboratory experimenters and astronomers are clearing up the complex spectra of the stars and are able on the one hand to connect them more and more with elements which we know, and on the other to help to extend the very foundations of natural knowledge.



Reverting, then, to the state of affairs at the outset of the nineteenth century : until the actual elementary *materials* had been discovered, any theory of their atoms, that is to say of the *objects* which are the structural units of matter, had necessarily been nothing but unverifiable guesswork ; but now the way was clear. And it ought to be specially emphasized that any such unitary theory to be put forward depended, and necessarily so, upon no merely qualitative study of the properties of elements and their compounds, but upon measurements of quantity ; and this applies both to the formulation and to the testing of the theory. Because mass was the one and only property of matter admitted as invariable, therefore the atomic theory had to be based on weighings.<sup>1</sup>

In the nineteenth century and up to the present day, the sequel to the discovery of a set of structural units of matter may be described as having three major components ; and at the same time the proofs that the discovery had been a genuine one were contained in that threefold sequel. In the first place, there was laid upon chemists and physicists the task of tracing the properties of material substances to the particles composing them : an analytical process. In the second place, the knowledge so gained had to be used for predicting and building up compounds anew : a synthetic process. Thirdly, the properties of atoms, disclosed by the first two processes had to be studied, once more analytically, with the result of referring these properties to still more primitive structural units within the atoms themselves.

Now all this comprises, not only the whole of chemistry, but all of modern physics too, since the study of energy as well as of matter is implicitly demanded by it. It is impossible at the present day to separate the spheres of physics and chemistry,

<sup>1</sup> It is worth recalling that the variability and " destructibility " of mass which are indicated by recent mathematics and physics, are confined to conditions very widely removed from those attending the interactions of chemical substances, in which, moreover, *ad hoc* researches have failed to discover any variations of mass. The above basis of the atomic theory is therefore safe and sure.

however the interests of their respective practitioners may diverge in the lesser issues of the atomic theory. The old comprehensive name "Natural Philosophy" might well be revived, for there is now only one Physical Science, to which physicists and chemists variously contribute; and perhaps the outstanding intellectual achievement of our time is that which enables such a statement to be justified. In the present survey of the evolution of chemical ideas, the vastness of the crop that has been reaped forbids the exposition of more than a selection from among the chiefest fruits. Merely to enumerate conclusions without giving at least a general guide to the logical steps that have warranted them would be foreign to the purpose of this book; and in this chapter we shall therefore dwell upon the consequences that have flowed most directly from the process already watched at work. That is to say, the survey will neglect almost entirely the magnificent outgrowth of synthetic chemistry; the vindication of the results of the analytical enquiry remains as the theme. Some of the results of unitary chemistry, together with an indication of their logical sanctions, are therefore presented a little further on. They are expressed as succinctly as may be done without the use of the technical shorthand terms that are apt to make one take too much for granted; and, in the language of the learned and reverend Dr. Watson of Cambridge and Llandaff, "Chemists must excuse me, as well for having explained common matters, with what will appear to them a disgusting minuteness, as for having passed over in silence some of the most interesting questions."

The desire to find Units whose assembly and interplay shall express things as we know them, is a mental necessity in any intellectual enquiry; and this analytical impulse, far from spoiling the beauty of the scheme of things, inevitably enhances it for us when it reveals the tangible unities and the rhythms which underlie, and indeed constitute, the huge complex of nature. Scientific men occasionally incur the reprobation of

their fellow-poets when they allow their analytic processes to be inspected at a halfway stage : the stage when that gracious unanimity of 'observables' which it is the artist's business to portray, seems to be riddled into discordancies and restless inward turbulence; yet there is no need for cavils and impatience, for the dissections of science in the end furnish always a plainer vision of absolute harmonies. Once more : "That school is so busied with the particles that it hardly attends to the structure; while the others are so lost in admiration of the structure that they do not penetrate to the simplicity of nature. These kinds of contemplation should therefore be alternated and taken by turns, so that the understanding may be rendered at once penetrating and comprehensive."<sup>1</sup>

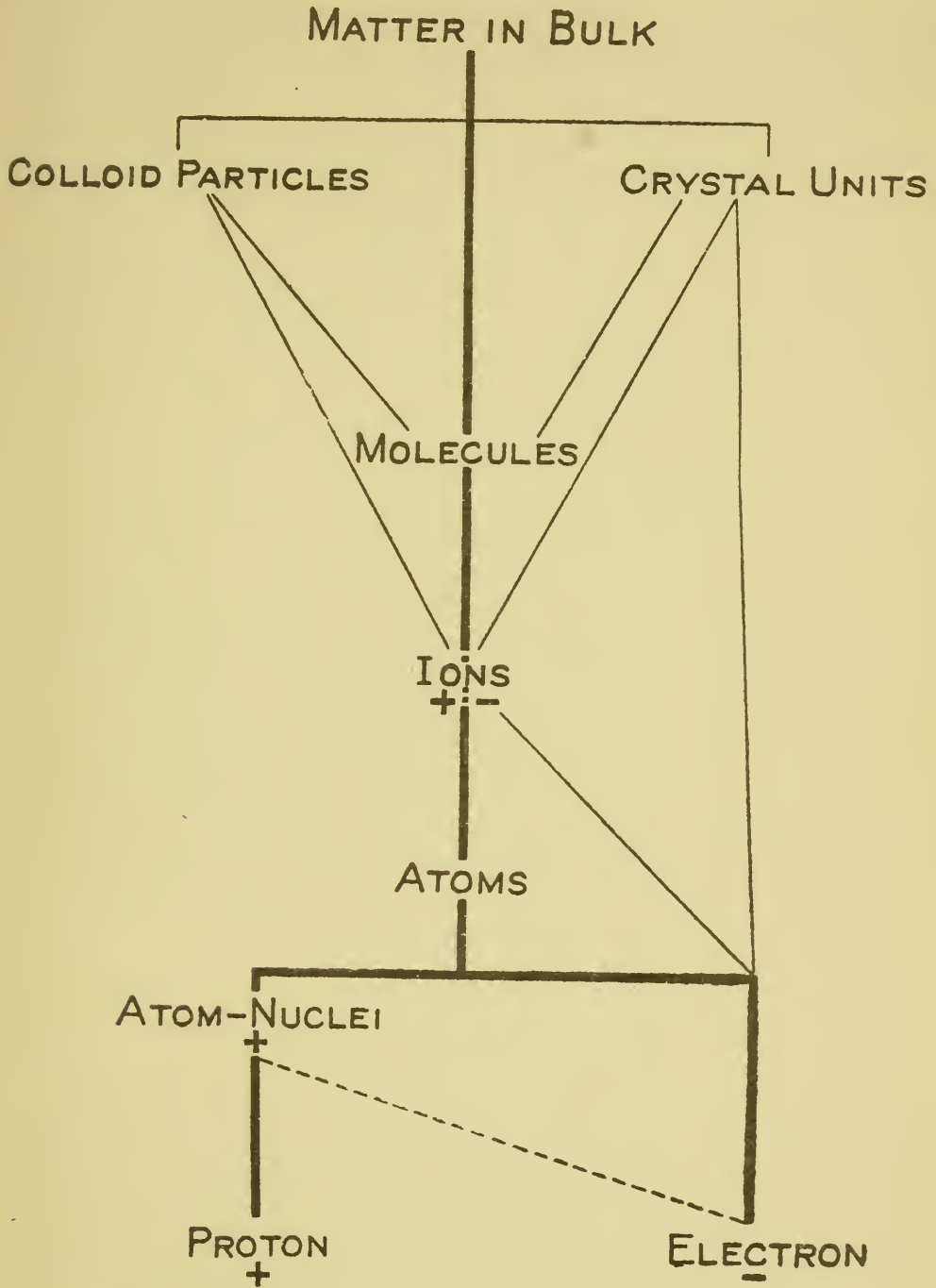
Accordingly, the leading theme to-day of physical science (including applied mathematics) may be taken as the discovery of a fundamental unit of matter; of a fundamental unit of action or energy; and ultimately of the cause-and-effect connection between these two kinds of unit. No one can presume to determine the deeper explorations of the future, but it seems justifiable to say now that a pair of the first kind of unit—the massive and positively electrified proton, and the weightless 'electron, the unit of negative electricity—is almost within our grasp. The second kind is perhaps drawing within range in the shape of the 'quantum' of radiation-energy, if indeed there be a real unit of what may prove only a secondary derivative of ultimate "matter" and the four dimensions. The definite capture of both kinds, however, must await the solution of the third problem, and this is as yet hardly above the horizon of the mathematical physicist.

When these three questions shall have received their first assured answers, there will be yet further probings of the infinitesimal, till the bounds of reality are touched. But accompanying all this, the great body of physical and chemical investigation will busy itself increasingly with what it has already begun to attempt : namely, with the task of learning

<sup>1</sup> *Francis Bacon*, see p. 19.



how to use the three new answers, as it is still learning how to use atoms; now in terms of them first to explain and next to



predict—verifiably—the behaviour of the progressively larger units, without whose prior discovery any reply to the three questions must have been idle and baseless. And as to those

discoveries for which inventors ask in the interest of human prosperity, they will accrue quite automatically during the spread of the new two- or three-letter alphabet, just as they have done during the present working-out of the 92-letter alphabet of the chemical atoms.

The whole enquiry seeks, then, to express the huge units of our stellar universe in terms of unit masses which we can finger and weigh; to express these, in their turn, in terms of molecules; molecules in terms of atoms; and atoms in terms of the proton and electron: and so by steps to link the largest with the least. The genetic results of the search are illustrated in the annexed diagram (p. 143); of those shown, the structural units for which chemistry stands sponsor are the colloid particle, the molecule, the atom, and the ion. It is the demonstration of the last three which is now to be reviewed. Only those features in their evolution which have remained steadfast are indicated; the history of chemistry during a goodly part of the nineteenth century was actually much more complicated than our study of it will suggest, for when the science flared up at the kindling of the atomic theory it became an eddying flux of discoveries large and small, transitory ideas, and the symbolic imaging of half-seen truths, in which every nation of Europe played a part; but from this welter there presently emerged the corpuscular theory in a form unshakably established.

## THE MOLECULE.

### (1) The Physical Molecule.

When cold water dries up by evaporating into vapour, it expands itself nearly forty-thousandfold, and this vapour can be expanded still more, without any definite limit; and it can intermingle with air or other vapours to any extent. We find it hardly possible to conceive of a *continuous* substance which could be endowed with such properties and still remain continuous, and we are compelled, in the face of these facts, to assign to water, and to everything else which shares such

properties, a "discrete" structure; that is to say, to picture it as being made of separate little masses which can spread far apart or can aggregate. These *molecules* must be very small, for no microscope will show them. A gas or a vapour, then, is provisionally pictured as being mostly empty, weightless space (whatever "empty" space is) with here and there pin-point massive molecules in it. In a liquid, the molecules would be quite close together; in a frozen solid, they would presumably be nearly touching and (in a crystal) massed in orderly ranks and piles.

Yet this is not all: for, despite the tenuous structure thus attributed to a vapour or gas, it has very definite powers of resistance, as everyone knows who pumps a tyre. By cooling the gas, it is made to shrink. The question thus is necessary: what is there which could keep the molecules apart, especially at higher temperatures? Do they merely hang in space? They might be thought—and have been thought—to exert upon each other at a distance some force of repulsion, whose strength increases when the gas is made hotter. This idea must not be lost sight of; nevertheless, it entails the invention of a novel agency, for whose existence there is no other evidence; and consequently, in accordance with the plain logic of common-sense, we must first see whether some more familiar agency will not fit the facts.

We therefore try Boyle's suggestion (cf. p. 97) that these molecules, not necessarily repelling each other at all, are in motion in all directions, as with a swarm of flies in a room. Each of our "flies," however, since from the outset we deny it free-will, must be presumed to dart on without turning, until it collides with others or with the walls of the vessel. What must be its fate after collision? Obviously, if it then stopped altogether—if the insects flew about in a room lined with a sticky fly-paper instead of wall-paper—the gas, as such, would in the course of time disappear from the middle of the vessel; but it does not, however long it is kept. Hence if there is molecular motion at all, it must be elastic motion; the molecule



when it hits anything must bounce off and resume its flight, and so on unceasingly. Quite likely, at any moment, some of the molecules are moving slowly, some quickly, retarded and accelerated by their own encounters; but there could be stated some average degree of speed or energy of movement.

Now this conception—the “Kinetic” conception—is useful in two ways to begin with. It enables us to visualize the mechanism of gas-pressure, which we picture as due to the steady drumming of a hail of molecules on the walls of the vessel. And it enables us to visualize the mechanism of temperature and the conduction of heat; for, whether the body be gaseous or not, it is easy to grasp the notion that the hotter it is—the more heat-energy it contains—the greater is the average energy of the molecular movements within it. Conduction of heat from one side of a body to the other is then the communication of a more energetic motion from each molecule to the next in front of it, by means of the collisions and impulsions which occur. That will partly explain why solids are better conductors than gases, for in them the molecules are closer and so collisions are more certain; and why also a “thermos” flask, from whose hollow walls all air has been extracted, will let practically no heat get in from outside nor out from inside, except by the narrow avenue of the solid neck; for between inner and outer walls are too few air-molecules to communicate much energy per second by their motions across the space.

Now, all this “kinetic hypothesis” can be put in a precise and mathematical way. So expressed, it was found to imply, as exact and necessary deductions, *all* those actual facts of gaseous behaviour which had first been summarized in the experimental laws of Boyle and Charles and Dalton and Graham. It was also successful in expressing a great deal of other work, when it was used in conjunction with the principles of thermodynamics, which themselves involve no sort of assumption whatever as to the inner structure of matter. On the same purely physical grounds, it was even possible to

infer that some gases are composed of molecules which must be in some way complex structures; to maintain the inward throbbings of which, a part of the heat supplied to the gas is automatically diverted. What these structures are, chemistry had already been called in to decide.

We therefore now hark back, to trace the other road which led up to this point.

## (2) The Chemical Molecule.

At the beginning of last century, Dalton studied the relative weights in which pure substances consent to unite together chemically. He discovered—to put it very briefly—that each sort of elementary matter behaves as though its chemical functions were discharged by particles whose special characteristic was the possession of an individual weight; which atomic weight varied in magnitude from element to element, but was fixed for each.

For the sake of those who wish to examine the deeper principles of Dalton's argument, the probable chief steps are concisely indicated: A weighable sample of any one element can be wholly turned into a pure compound with another only when its partner has been provided in a definite quantity. The relative proportions hold good for this particular compound however small may be the weighed quantities used for the experiments. Therefore these relative weights must obtain within the least imaginable bodily fragment of that compound that could function chemically at all. Next: when a second pure compound of the same pair can be made, Dalton found that an extra dose of one partner must be provided which turned out to weigh exactly as much again as the first dose of it already present in the former compound (or in other cases exactly twice, thrice as much, etc.). Consequently the least imaginable fragment of this second compound has exactly twice (thrice, four times, etc.) as much of the "partner" element in it as the least fragment of the former compound had. This peculiar *numerical simplicity*—the existence of a Highest Common Factor—was true of all cases. The inference is drawn that the stuff of the partner-element can function *only* when it is made up in a package having one particular weight; one, two, three such packages being what forms part of the least imaginable compound particle; but no fraction of a parcel, nor any larger *and single* parcel, can do so. These fixed and constant parcels are thus the individual units which embody the chemistry of that element: its atoms.

The same "H.C.F." or relative weight is found to obtain for a given element when it is partnered with a succession of different other elements. Each definite element therefore is characterized by a distinct and definite atomic weight.

The Atomic Theory, now for the first time put forward in a form amenable to experimental test, gave to chemistry the status of a unified science; great numbers of known facts became linked, and the accumulated knowledge of a century and a half, founded on the labours of Boyle, Black, Scheele, Priestley, Cavendish, and Lavoisier, was ready to be knitted into a single structure.

Yet, before uniformity could be properly achieved, and while the immediate indications of the atomic theory were being explored, it became necessary to clarify the concept still further; and naturally, this demanded still more experiments. For, as yet, the connection between these atoms and the larger units of which they are the parts, was not sufficiently clear; the kinetic molecular hypothesis was not to be brought forward till long after. Gay-Lussac, measuring the proportions in which gases will combine chemically, found that the volumes of any two combining gases are always either equal, or else extraordinarily simply related, *e.g.*, as 1 to 2 or 1 to 3. It was Avogadro who pointed out that the only sensible interpretation of this chemical fact is, that a given volume of any gas consists of a number of particles, *which number does not in the least depend on what variety of stuff the particles are made of nor on how large they are*. Thus, in forming water, every 2 litres of hydrogen are found to join with 1 litre of oxygen; and this peculiar simplicity, paralleled by any other cases of gaseous combination, must mean that the union of the gas-samples is simply a manifold repetition of vast numbers of individual unions, each of which occurs between 2 ultimate particles, molecules, of hydrogen and 1 of oxygen. (It will be noted that the general type of this inference of molecules from volumes is of the same sort logically as was Dalton's inference of atoms from weights.) When, moreover, it was



found that the volume of steam formed from one measure of oxygen is just two measures, it followed that out of every single molecule of oxygen there had been formed two identical molecules of water. That is, the physical molecule of oxygen had been split into two, each half joining with a whole hydrogen molecule to form a whole molecule of water. These two halves are the chemical atoms of oxygen, whose molecule is built of them. This train of logic is probably the most fundamental part of the whole atomic theory.

It was the fusion of the chemical molecular hypothesis of Avogadro with the atomic theory of Dalton which supplied a really firm basis for chemistry and rescued it from a condition of unhappy entanglements. Curiously enough, although Avogadro's hypothesis was put forward within three years of Dalton's, that fusion was not effected until nearly fifty years afterwards, in 1858, at the hands of Cannizzaro. Certain of the chemical results will be indicated in a later section of the chapter; meanwhile, it can be said that they were so far-reaching, and in themselves carried conviction to such a degree as to fortify almost to the point of impregnability the two theories from which they depended and which at the same time they more richly expressed.

At about the same time as this began to take place, the physical or kinetic theory outlined in the earlier paragraphs took shape, in the hands of Clausius and Clerk Maxwell. As we have seen, it was at once fully successful in explaining the then-known facts of the physical properties of gases; but it did more, for the physical kinetic theory was shown necessarily to imply the identical hypothesis which Avogadro had promulgated on chemical grounds. Thus, gaseous physics and the whole of chemistry could now rely upon each other's full experimental support in their joint conception of the molecular unit, which could henceforth be called the physico-chemical molecule.

We may for clearness describe the later work on the molecule in two sections, which strictly speaking overlap in point of

time, and bring us up to the present day. Much must be omitted, or barely mentioned; and nothing like justice can be done to the skill and courage of the experimenters; all one may hope to convey is a little of their way of thinking.

### (3) Weighing and Counting Molecules.

(a) *Relatively*.—With the advent of Avogadro's hypothesis, it became possible to find by experiment how many times heavier a single molecule of one gas is than that of another. If we were to weigh two heaps of coins, one heap of florins and one of halfpennies, and we found, let us say, that the florin-heap weighed 2.1 times as much as the halfpence-heap, this alone would not allow us to say how heavy one florin is compared with one halfpenny. But if we were then told that there had been the *same number* of coins in each heap, then (without having to know what that number actually was) we should at once say, one florin is 2.1 times as heavy as one halfpenny. So it is with gases; for Avogadro shows us that we shall get equal numbers of molecules to weigh, if we choose equal volumes of our gases (at some one fixed pressure and temperature).

In this way the molecular weights—that is to say, relative molecular weights—of different substances are measurable. Since a litre of nitrogen has 14 times the weight of a litre of hydrogen, therefore a single molecule of nitrogen is 14 times as heavy as that of hydrogen. When, in addition, chemical tests and measurements have shown what elements, in what proportions, are present in these substances and therefore in each of their separate molecules, we can say how much the constituent elementary atoms of a single molecule weigh. It is all *relative* so far; for instance, the molecule of water (steam) is nine times as heavy as the molecule of hydrogen, and in either of them there turns out to be twice as much hydrogen as the smallest amount of hydrogen found present in any other molecule. This least quantity defines the atom

of hydrogen, H; and because it is the lightest atom known, its weight may be provisionally called 1, until we know its weight in ordinary units. To the chemist, the symbol "H" does not mean simply the substance hydrogen; it means one atom of it, weighing 1. Hydrogen gas is made of molecules each of which we write " $H_2$ "; the water molecule, containing this quantity and one atom of oxygen, is the object symbolized by  $H_2O$ . These symbols show *amounts*, then, just as much as 2s. 1d. does; in fact, there is no particular reason why we should not write this molecule of money as  $S_2D$ . We have in the foregoing a very short summary of that fusion of Avogadro's hypothesis with Dalton's which has already been mentioned.

The experimental methods for finding the relative weights of gases are very simple in principle, but not so easy in practice, as Galileo, Bacon, and Cavendish had all had to realize; for the gases are naturally very light compared with the vessels in which they must be weighed, and any small errors in the accuracy of the weighings therefore bulk largely. Morley in America, the late Lord Rayleigh, Guye at Geneva, Whytlaw-Gray here, have in turn developed the methods so that with ordinary gases such as oxygen, nitrogen, nitric oxide, and the like, we can now trust some of the relative gaseous densities as being accurate within two or three parts in ten thousand. Methods have also been developed so that, where a substance becomes gaseous only when strongly heated, it can then still be weighed, indirectly; and such methods have been used up to  $2000^{\circ}C$ . An interesting example (at ordinary temperatures) was when, with exceeding skill, the late Sir William Ramsay and Dr. Whytlaw-Gray used the microbalance of Steele and Grant to weigh the gas Niton or radium emanation; the quantity available weighed one-thousandth of a milligram, and its volume was about a tenth of the size of an ordinary pin's head.

(b).—The next step towards counting molecules was made in a great expansion of work, which enabled the operations of molecules to be traced elsewhere than in gases—hitherto



the only physical state of matter in which this could be done with any certainty. It was, of course, recognized that molecules are everywhere present, in liquid solutions as well as in gases or anywhere else; but the advance consisted in the proof that the dissolved molecules in *solutions* confer upon those liquids a group of properties which enables the relative numbers or weights of these molecules to be counted, as if they were molecules in a gas. For example, the tendency of water to evaporate into vapour, and its tendency to freeze into ice, are found to be lessened when a foreign body is dissolved in it. The solution has to be made hotter to boil it and colder to freeze it. Again, pure water tends to diffuse, molecule by molecule, into a watery solution, if this is placed in contact with it in such a way that ordinary rough mixing does not occur. The fewer the water-molecules in the solution—that is, the more numerous the foreign molecules—the greater is this tendency; and if the swelling of the solution by interpenetration of the water is to be prevented, mechanical pressure—known in this connection as “osmotic” pressure—has to be exerted on the solution. All these properties of solutions, then, can be measured and exactly expressed; and in a great mass of diverse work which was brought together by van 't Hoff at Leiden in 1887, it was shown that one and all depend upon the *numbers*, not at all on the *kinds*, of the foreign dissolved particles in the liquid. We are forcibly reminded of Avogadro's and the kinetic theory of gases; the parallel is actually of a close and quantitative kind; so that evidently here likewise is a means of relatively weighing or numbering dissolved particles.

One outcome of this was to be the discovery of ions, told in a later section; the other which now concerns us is as follows: for clearness it is told in a very much simplified form.

(c).—It has been found that the foregoing “osmotic” effects are shown, not only by solutions, but by *suspensions* of visible particles in otherwise pure water. In this case it is possible to count the foreign particles by eye, assisted by

the microscope and, better still, by the ultra-microscope; and it turns out that the "osmotic" effects depend, once more, on the *number* of the suspended particles, in exactly the same way as with dissolved molecules and with gaseous molecules. It is these suspended particles which are the colloid particles referred to in the diagram on p. 143. It may be added that whilst the colloid particle is known best in suspension in liquids, it is also represented in gases by mist and by dust. It has been found to owe its peculiar properties (which include the possession of a charge of electricity opposite to that of the neighbouring medium in which it swims) to a special arrangement of the molecules which ensheathe it. The colloid particle is intermediate between the small units of physical science and the cells and micro-organisms which are the small units of biology; and there is no physiological process in which the behaviour of colloid particles does not play a highly important part.

Recurring to the physical examination of suspensions, we learnt that these tests exhibit visible mud-particles as being in no way different from gas-molecules; and, since they can be counted by eye, we are provided with a means of finding the real number, not merely the relative number of them and, inferentially, of the molecules in any gas.

Actually, the molecular nature of suspended particles is even more convincingly shown; for they can be seen to be in motion, hither and thither, in the fluid. This long-known "Brownian" motion has been timed and measured by various means, including the use of the cinematograph; and it turns out to be precisely that which, on the kinetic theory, would be given by magnified molecules having the size and weight of these particles, moving through a liquid, and bombarded by the (invisible) agitations of the liquid's own molecules. One might say that as a result of this twentieth-century work, due notably to Perrin in France and to Svedberg in Sweden, the presence of the liquid's own molecules, and their movement, have been proved to be as real as the dark stars'; and we are

now no less firmly assured of the true existence of molecules in general than an astronomer would be assured by a personal visit that Mars is truly a planet.

Measurements of this sort have been confirmed by a large number of investigations of other kinds. Some of these, as in Rankine's experiments upon gaseous viscosity, are made with materials in the ordinary molecular condition of gases; others, as in the researches of H. A. Wilson and of Millikan upon the movements of electrified mist-droplets, have had to do with aërial colloid particles and with electrical measurements. All the results converge upon the same conclusion, so that we now know with very fair precision the actual numbers and the approximate sizes of molecules.

In a drop of water there are some two thousand million million million molecules. This conveys no conceivable idea; so we may put it in another way. Supposing that we were to magnify one drop of water to the size of the earth, and all its molecules and their three component atoms proportionally, we should see a space populated by something like trios of toy-balloons, bouncing about everywhere at intervals of no more than two or three feet.

Another illustration: with modern appliances it is possible to exhaust air from a vessel (*e.g.*, a wireless valve) with such thoroughness that the vacuum is said to be perfect, and the pressure of the infinitesimal trace of residual air approaches only one ten-thousand millionth part of ordinary barometric pressures; yet in every cubic centimetre of this, the most nearly "empty" space that we can produce, there are still flying three thousand million molecules of air.

That it has been possible to prove anything at all, let alone to prove anything precise, about objects so minute as molecules, is no doubt wonderful enough; but the real miracle is the molecule, not our methods of divining it. The thing is so small, and yet it decides the behaviour of bodies so great, that it lies far beyond the limits of insignificance and is even awe-



inspiring; and still more so when it is realized as being formed from things even smaller.

#### (4) Molecular Attractions.

(a) *The Liquefaction of Gases.*—The obvious fact that some gases are more easily liquefied than others—for example, compare steam with air—shows that some molecules tend to cling to their kind and form aggregates. No gas known at the present day has resisted liquefaction. Many gases, such as laughing gas, carbonic acid gas, and others, can be liquefied without cooling them, by simple compression. Others, the “permanent gases,” like air, hydrogen, and so forth, have been compressed up to 20,000 atmospheres—100 to 150 tons per square inch—without showing any sign on the way that there was any *separation* of liquid, even though the very molecules were compressed and distorted. But even these gases liquefy by pressure, provided that they can first be cooled sufficiently.

It was Andrews, in Belfast, who in the 'sixties first showed what he called the “continuity of the liquid and gaseous states,” and demonstrated the existence of a ‘critical temperature’ for each gas, below which it can be liquefied by pressure, above which it exists only in one form, more or less dense. Thus, below  $365^{\circ}$  C., water can be all liquid, partly liquid and partly vapour, or all vapour, according to the pressure on it; above  $365^{\circ}$ , it can be nothing but gas. One half of the principle of the liquid air machine is that whereby the air must not only be compressed, but must be compressed when it is below —  $140^{\circ}$  C.

Van der Waals next showed, by a modification of the kinetic theory, that the existence of a critical temperature, and, in general, the new facts which Andrews and his successors discovered, can be accounted for by allowing for two things, neglected in the simple physical theory; namely, that molecules have size, and that they tend to stick together when they are close. This ‘cohesion’ works, of course, against

the independent motion which tends to keep them apart. It is not necessarily quite the same as the force which makes one atom combine with another chemically; at all events, we find cohesion even among the gases of the argon group, and the atoms of these are not known to form any ordinary chemical compounds at all. Although various ideas are current as to what might be the origin of molecular cohesion in gases and liquids, they are still under test. No sort of explanation of cohesion is therefore justifiable here; we shall accept the fact and leave it at that.

We may note at this point one or two uses which are made of this cohesion. Evidently molecules can cling together with a good deal of energy; and correspondingly, it requires the using-up of a good deal of energy to draw them apart again (as is manifest when one's wetted finger grows cold on the windward side, for instance). When a compressed gas which contains cohering molecules is suddenly released so as to expand, its molecules are compulsorily drawn apart; and the resulting using-up of energy, necessary to provide for this separation, makes the whole gas cold. The liquid air machine makes use of this method of chilling in order to bring the air to the pitch of cold at which it can begin to liquefy—its critical point. With the help of liquid air which is made to boil away at  $-200^{\circ}$  C. in a cooling-vessel, even hydrogen can be chilled enough to make it liquefiable; and with the aid of liquid hydrogen in turn, the most "permanent" gas of all—helium—has been liquefied by Kamerlingh Onnes at Leiden. Onnes has used this liquid helium to study the behaviour of materials at the lowest temperatures yet known; he has reached within one degree of the absolute zero of cold,  $-273^{\circ}$  C. At such a temperature, " $1^{\circ}$  Absolute," every substance except helium is rigidly solid; and the molecules are outwardly all but lifeless. It is not improbable, from what we otherwise know of its molecules, that helium may prove to be incapable of forming anything nearer to a crystalline solid than a sort of loose-knit and flexible assemblage responding only to certain optical

tests for solidity; but this is a speculation which only those who can test it are properly entitled to make.

(b) *Liquid Films*.—The foregoing variety of cohesion is not one of whose inner mechanism we form a very definite picture; but the explanation of it will doubtless turn out to be akin to, if not the same as, that of several better-understood phenomena, which we shall now very imperfectly indicate. Their study belongs mainly to this century.

When a liquid forms drops, each of these is held together, and its size is decided, by the tautness of the skin of the drop. This tautness can be exhibited and measured in a number of experimental ways, some of them very delicate in detecting changes in it. The pouring of oil on troubled waters is our most ancient example of an artificial change in 'surface-tension.' The late Lord Rayleigh studied quantitatively this effect, and measured the least quantity of oil which was necessary to affect the surface tension of water. He showed that the minimum layer is so thin that it seems to be a carpet no thicker than one molecule. Hardy, working by another method, showed that the energy with which films of different oils adhere to various surfaces is definitely connected with the presence of certain chemical atoms or atomic groups within their molecules; and his work, emphatically confirmed by many other recent investigators, leaves little doubt that, in such films, the molecules are anchored to the liquid or solid surface to which they cling, by reason of forces emanating from certain special atoms in them. Each molecule is, as it were, more sticky at one part than at another; and the whole film on a solid is like a mussel-bed on a rock. (The same thing happens when gases or vapours are absorbed with porous materials such as charcoal; and on this theme there hangs much of that great part of chemistry summed up as "Catalysis," the hastening of chemical action by the presence of materials which offer seats of abnormal activity.) Certain of the atoms, though bound to each other within a molecule, can evidently spare some of their force to reach out a little way and attract



similarly-disposed atoms in neighbouring molecules; as when a rower joins hands with someone on the landing-stage and so makes fast the boatload. We cannot doubt, in the face of this work and of a great deal of strong collateral evidence, both chemical and physical, and too voluminous to be indicated here, that at least a part of molecular cohesion or attraction takes its origin in the incompletely-monopolised affinities of one or more atoms within the molecule. It would be premature in this place to push the matter deeper.

(c) *The Solidification of Liquids*.—It is characteristic of solid crystals that they influence in a peculiar way the passage of light through them. The effect which is well seen with Iceland spar is one example; if a clear crystal of this is held at a proper angle and used as a window through which to view, for instance, a ruled line, the line is seen doubled. The effect is called "double refraction." We cannot escape the conclusion that this has to do, somehow, with the *orderliness* of the molecules and atoms in crystals; an arrangement long believed to exist, and precisely shown as a fact by the beautiful researches of Sir W. H. Bragg and Professor W. L. Bragg upon X-rays, starting from the pioneer experiment of Laue. The effect produced by a crystal upon a beam of light is thus thought of as the summation of a number of successive small effects, each contributed by one small molecule or unit, and all working in the same way because all of the molecules or units of the crystal are facing the oncoming beam in the same way. When the molecules, or such parts of them as bend the light, are higgledy-piggledy as they are in a gas or a liquid, double refraction is not observed, because on the whole there are at every instant just as many molecules acting in one way as there are molecules acting in the opposite way. The essential thing is, that double refraction implies a preponderance of *order* in the molecular array within the doubly-refracting body. Adopting this test, we may see what it tells.

Gases and liquids show no orderliness. Also a glass shows no optical difference from a liquid; for glasses, and what are

called "supercooled liquids" in general, are simply liquids which have been cooled without freezing till they are so viscous as to resist, perhaps for years, any internal settling of their molecules into a symmetrical pattern. As Robert Boyle put it in a flash of insight 260 years ago, "the particles of the Glass agitated by the heat, were surpriz'd by the cold before they could make an end of those motions which were requisite to their disposing themselves into the most durable texture." If a supercooled liquid such as glass is kept fairly hot, it becomes frosty, or "devitrifies"; the molecules have been sufficiently released to set into crystals.

Optically, all 'amorphous' transparent bodies, such as celluloid, jellies and colloid "solids" are like glass. Nevertheless, if glass or celluloid be strained mechanically along one direction, then, while the strain lasts, there is double refraction; to some extent the molecules have been forced into lines rather as if one took a tangled bundle of chains and pulled it out taut. Glass which has not been properly annealed in manufacture is under internal stresses of this kind, due to unequal contractions during cooling, and so is liable to fly in pieces; and the examination for double refraction (by a very sensitive method) is now applied as a routine test of good annealing in many glass-works. Ordinary liquids, too, can show the effect of strain; but since their molecules are more mobile than those in glasses, and are consequently less easily "surpriz'd" before they slip back into random motion, the stress has to be applied rapidly. Thus, oil which is lubricating two quickly-moving surfaces is found to be in this condition. So also, suspensions of certain rod-shaped particles (iron oxide, vanadium oxide) in water show double refraction if the rods are made to "head" in one direction, either by streaming the liquid rapidly along a tube, or by placing it between the poles of a strong magnet.

Now, certain substances have been found which *spontaneously* become doubly-refracting whilst still in the liquid state. When well above their freezing points, such bodies are

quite normal liquids, optically; but on being cooled to near their freezing points, they show, under the special illumination used in the test, places of double-refraction; and yet any foreign particles which may be present can wander freely through these places, so that these are still mobile. On cooling further, the liquids freeze to ordinary solid crystal masses, retaining and extending their double refraction. It seems from this that "liquid crystals" are places in the liquid where the molecules have begun to string themselves out in anticipation of the array which they will ultimately assume, though in closer formation, in the solid crystal. Evidence of this preliminary "stringing-out" has also been found in liquids which are on the point of forming a fibrous curd.

(d) *Change of Solid Form.*—We might have thought that, arrived at the solid crystalline state, the molecules might be finally at ease; yet it is not always so. There are innumerable cases in which one and the same chemical compound or element exists in more than one solid form. The classic example of carbon, in the diamond and in graphite, is only one of many examples of this 'allotropy' among the elements. Tin, when it is kept in cold climates, tends to suffer from "tin plague," in which the bright tenacious metal falls away in pits and patches to a grey powder. The powder is still pure tin; and it has been proved that if this is kept warm it can be made to revert to the ordinary form, though no longer, of course, in a compact shape. Iron has several forms also, and only one of them is magnetic. An interesting compound, ammonium nitrate (the chief component of our war explosives), can change into another and more voluminous form of the same substance so quickly, when kept rather warm, that it breaks the glass vessel which holds it. Hundreds of cases might be cited, many of them involving marked changes of colour as well as of other properties.

In many of these cases we can point to a definite temperature at which the transition or molecular rearrangement occurs, and this is no less definite than the melting-temperature.



Often, also, heavy pressure assists in the production and the maintenance of the change; if the form to be produced is less voluminous than the original, pressure inevitably helps. It is interesting that in the last few years ice has been proved to undergo these changes, when suitably compressed. There are no fewer than five "ices," each form with its own distinct properties, and each best fit to exist within its own characteristic range of pressures and temperatures. One kind of ice which requires pressures over 40 tons per square inch to form it, exists—and can exist only—at temperatures higher than "freezing-point," *i.e.*,  $0^{\circ}\text{C.}$ ; and, with pressures such as 100 tons to the square inch, it has even been kept, perfectly solid, at  $75^{\circ}\text{C.}$ , a temperature such as can hardly for a moment be borne by one's hand. The possible bearing of these results upon glacial geology is perhaps worth thinking about; for they limit the pressures and at the same time extend the upper ranges of temperature at which solid water can exist. The influences of pressure upon the behaviour of mineral compounds in general must have an important bearing on many geological problems.

Regarding the nature of the linkage between molecule and molecule in a crystal, the study of the crystals of many organic compounds has shown that separate molecules tend to come particularly close together at points where each contains an atom of certain special elements. It is to be inferred that the linking of unit with unit in a molecular crystal is determined, as in films on surfaces, by stray forces emanating from atoms within the molecules; and the above-mentioned specially-active atoms are of the very kinds which had long been known, from chemical evidence, to favour the union of one molecule with another. The crystals cleave or split most readily along the planes where atoms known chemically to have weak "residual affinity" are facing each other. An analogy with the splitting of a bread-and-butter sandwich is not as inexact as it may seem; for ultimately, the same forces join the components of a sandwich as of a crystal.

(5).—We may draw this section to a close by describing the intimate mechanism of the passage through the three states of matter which occurs when we cool a vapour. The picture here drawn, it should be remembered, is entirely based on exact numerical measurements, and describes nothing which is not authenticated by them. We begin with the gas enclosed in a vessel of some kind.

The gas is chiefly space, and in this space innumerable molecules constantly fly, collide, and rebound, spin and internally vibrate; if, here and there, small pairs or trios are formed, they are instantly dismembered by the onset of molecules whose paths they cross; no molecule is ever at rest for more than an infinitesimal part of its perpetual journeying.

As the gas is cooled, the movements diminish in vigour; the transient clusters may form more frequently, last a little longer, and grow a little larger, but within each cluster the loosely tied components are violently vibrating, ready to fall asunder at the first impact. Meanwhile, over the walls of the vessel there has spread a sheathing of molecules, all anchored to those which jut from the solid wall, and serving as targets to others which shoot from the mass of the gas. With further cooling the coating thickens, as layers of molecules pile up on the first; the clots increase in the gas; and we are at the point when, with a touch more of cold, the clots thicken and merge together from out of the space and, gathering into one loose body, cling to the layers lining the wall; the gas has formed a liquid.

All through the liquid, incessant oscillations, jostlings, and slippings-past still go on; and at the cordon of the upper surface, molecules are continually breaking away or piercing the rank to escape into the vapour-space above, and are as continually returning.

As we cool the liquid, the oscillations grow quieter, the mass becomes more compact, fewer molecules remain in the vapour-space and fewer break from the surface; more and more frequently, groups of molecules within the liquid find themselves

close enough and suitably-set to begin to join in regularly-ordered chains and figures. If at this point we suddenly cool the liquid extremely, the molecules are immobilised where they stand, and in this state of suspended animation they constitute a glass or a jelly; and at a number of points in it there are the groups or 'crystal nuclei' which await only the release of the constraint to continue the orderly building which they had begun. Allowing the super-cooled liquid, then, to grow warm again as it was before the sudden chilling, we renew the state of mobility; and now, with the resumption of gentle cooling, there is reached a temperature where the energy of propulsion can no longer debar the inward atoms of the moving molecules from maintaining their directing drag upon others which pass within range; all the molecules are pulled into positions of quivering equilibrium, dwelling in serried rows of habitations, and the liquid has frozen into a crystal.

Only by further cooling, and perhaps by pressure, may any rearrangement be made; though this happens with less and less freedom as, with lowered temperature, the quiverings diminish, and so perhaps the chance is less that the right parts of neighbouring molecules shall be favourably placed to bear mutually on one another; till at length the particles are locked into whatever places are most narrowly and symmetrically fitted to the shapes of their 'spheres' of influence. But in no case will they cease to vibrate, each within its own confines, until the crystal reaches the unattainable absolute zero of cold.

### THE ATOM.

For the sake of clearer understanding, we shall diverge from the order of units shown in the diagram on page 143, and shall treat of the atom before the ion.

From the fusion of Dalton's atomic theory with Avogadro's and the kinetic molecular hypotheses (page 149), there came a confidence in the existence of atoms as objects and not as little more than arithmetical counters, which soon won unanimity



among chemists. This assurance led, during the second half of last century, to two discoveries, of the highest importance then, and at the present time in the very forefront of interest. These were, the property of Valency, and the Periodic Classification. Only so much of these need here be said as is necessary for the development of our present theme.

(1) **Valency.**—As a result mainly of the study of compounds of carbon by a number of workers, culminating in the researches of Frankland, a new intrinsic and numerical property was discovered in atoms. Hitherto, the measurable properties which had been assigned to atoms had been of a kind shared by any sort of body: mass, volume, specific heat, are familiar and measurable in bulk-matter, even though we knew nothing of atoms. The new property, however, is characteristic of atoms and of atoms alone; it is the one feature which we might have marked as distinguishing chemistry from everything else, but for the barrenness of any such discrimination.

The valency of an atom is the measure of its capacity to unite with other atoms within a molecule; it describes how many such other atoms it will link itself with, without regard to the intensity of the linking force. The essence of the discovery was that valency is a property which goes *per saltum*, in units. The way was prepared for a simple classification of elements according as their atoms have a valency of 1, 2, 3, and so on up to 8, this being the maximum valency which it was found necessary to invoke.

At first a merely arithmetical property of atoms, valency was later exhibited by Couper and Kekulé, and especially by van 't Hoff and Le Bel and their successors, as a geometrical property. For the first time, there was produced reason for regarding the atom as something more than a mere lump, and a glimpse was given of its having a characteristic shape; or rather, of a characteristic orientation of the forces emanating from the atom. The story of the development of geometrical valency, and with it the study of the intensity-factor of inter-atomic linkages, belongs to the detailed history of organic chemistry;

for it has been the specialized chemistry and the physical properties of the compounds of carbon which have supplied by far the clearest information on these aspects of valency. All of this great field of chemistry needs its own proper historian.

Chemical reactions alone, however, cannot afford a knowledge of what actual objects or structures inside the atom produce the outflung attractions which link it with others. Sooner or later, the enquiry into these underlying causes was bound to begin; and this became possible only when the nature of ions began to be understood, and when the existence of electrons had been disclosed. Further reference to this is made in later paragraphs; meanwhile, it will be noted that here is the beginning of that application of the new "three-letter alphabet" which was indicated early in this chapter. The application is proceeding along two convergent lines, both of which started from the knowledge of atoms afforded by Sir E. Rutherford and clinched by Moseley's research: that is, that an atom has a positive nucleus around which are negative electrons, whose number is known for each sort of elementary atom.

On the one hand, Professor Niels Bohr of Copenhagen, led by the facts of the spectroscopy of light and of X-rays, has devised an atomic model in which certain peculiar changes of station on the part of the electrons are held responsible for radiation effects; and so precisely has the mechanism accounted for observed results and correctly predicted others, that the probability of its containing much of the truth is high. And he has been able to elaborate the matter, guided by the chemist's Periodic Classification, so that the 'Bohr atom' is already being used deductively with some success in chemistry to explain facts which had already been classified and to account for some which had not.

On the other hand, numerous chemists have preferred to approach the matter differently. Putting aside, for the time being, the outcome of work on physical radiations, they

have begun by acknowledging that planetary electrons, somewhere in the Rutherford-Moseley atom, decide valency and other chemical properties; and they have proceeded to devise stationary arrangements of these electrons which could give a reasonable picture of chemical behaviour. Professor G. N. Lewis, of California, has been particularly successful in correlating large numbers of diverse old and new properties by way of his models, and has provoked much interesting work. Again, organic chemists, who have gained a special insight into the peculiar variations of the forces of valency, have recognized these forces as having an electrical origin, and some are seeking to deduce the movements of a few electrons among the atoms composing the molecules of carbon compounds. All this represents the situation broadly; there are many shades of idea according to the particular types of atomic property under consideration.

It is very clear that the devising of a genuine atomic model must rest with the mathematical physicists, who alone have the quantitative engines for its construction and can deal with the factors which dominate its intrinsic stability; yet the inspiration of qualitative hypotheses on the chemical side is bringing forth a wealth of new information and detail which will eventually have to be incorporated in the exact theory. The point to be noticed is that chemistry has been offered a new unit, the electron, to build its atoms with; and the first result is distinctly reminiscent of what happened at the corresponding time in last century when, having found for itself a new unit, the atom, to build substances with, chemistry assimilated it only after many tentative and partial attempts had been made. Such a period of lag is, of course, a source of strength, not a sign of weakness; for it is in the rough-and-tumble usage to which the unfamiliar unit is subjected that its real capabilities are brought to light and its imaginary powers are gradually discredited. With the host of physicists and chemists now active, the probation of sub-atomic chemistry



should be over in considerably less time than was required for the maturation of atomic chemistry.

(2) **The Periodic Classification.**—(a) The newly-certified atomic weights of the 'sixties provided a sequence of numbers whereby all the then-known elements (about 60) could be arranged in order, so that the trend of their properties with rising atomic weights might be examined. An English chemist, Newlands, half saw and in his "law of Octaves" in part enunciated the ensuing generalisation, which was fully proclaimed by Mendeléev.

The word "octaves" gives the clue to the nature of the discovery. Just as on the piano we find that with rising pitch we get a periodic recurrence, in due order, of notes so similar as to receive the same names from A to G, so with rising atomic weight as we pass from element to element, we find a periodic recurrence of similar elements, and all these fall into groups named from I to VIII. Broadly speaking, all the elements within any one group have the same valency, and there is a regular gradation of other properties, from the member with the lightest atom to that with the heaviest; while between one *group* and the next there are all those chemical differences which are associated with the stepwise increase of valency.

The sequences initially suffered from many blemishes; but Mendeléev had formulated, and was confident in, a brilliant hypothesis of rhythm; and he boldly stated that the blemishes were due on the one hand to faulty assumptions made by early experimenters, and on the other to elements as yet undiscovered, for which places should be left vacant in the sequence. From the existing gradations of properties within one group and from one group to the next, he stated in exact figures what the data alleged to be incorrect should be, and he foretold in precise detail what the missing elements and their compounds should be like, both physically and chemically. Re-investigation of the antiquated data by fresh workers proved him right in that respect; and within twenty years the discovery of three

new elements, gallium, indium, and germanium, which exactly fulfilled what he had predicted for them, triumphantly vindicated the classification and the doctrine of rhythm.

To review all the properties which were "tidied up" and correlated by the Periodic Classification as time went on, would be neither possible nor desirable in this place. This systematic study constitutes what is often still called Inorganic Chemistry, although the usual purview of inorganic chemists has by now extended far beyond the stage of simply classifying chemical properties, and has merged almost completely into the scope of general (including physical) chemistry as it is described in the introductory section of this chapter.

We can only note, then, that the periodic recurrence is seen in such properties of the elements as:—metallic and non-metallic nature; ease or difficulty of melting and vaporizing; magnetic susceptibility; specific gravity; and among the chemical properties, valency; combustibility; ease of corrosion by various chemical agencies; ionic characters in general; whilst their chemical compounds show periodic changes in physical properties, in solubility, in acidity and alkalinity, and in a great number of other properties, all exactly measurable.

What ought to be specially noted here are rather the major factors in the array of the elements, which later work has brought out.

One of these may be expressed by a quite rough analogy with the piano keyboard once more. Just as, in ascending the gamut of notes, we find the simple scale in the key of C major to be invaded by black notes which constitute a breaking into the regular sequence, although each of them bears a relation to one of the white notes in the scale, so also in ascending the gamut of the elementary atoms, we come to invasions into the most simple-seeming sequence. The atomic "black notes" are, however, massed together without any alternating "white notes," so that they form a set of what are called the sub-groups in the Classification. Each sub-group echoes to some extent the characteristics of a main group to which it corresponds.

It is as if we had to do with what the musician knows as harmonics; and the development of sub-atomic structure has shown that this notion is more than merely fanciful, and that we may look forward with some hope to an expression, in terms of the vibratory parts of atoms, of the relations between main and sub-groups; and even of that interesting case of harmonics nearly in unison, which the chemist knows as the "rare-earth elements."

(b) The second important matter is the discovery of a certain set of elements, which formed an entire group of the Classification, and whose existence had been quite unforetold. This was the helium-argon group of gases, whose discovery by Sir William Ramsay with Lord Rayleigh on the one hand and with Dr. Travers on the other, forms one of the most interesting and dramatic stories of chemistry. The reason why no provision had been made for this whole group in the chemical classification became evident after their investigation, for they proved to be devoid of all chemical properties. Yet, now, those elements are absolutely essential to our ideas of why ordinary elements undergo chemical changes at all; for we see in the electronic structure of the atoms of the 'inert gases' the perfect and completed types, whose forms the atoms of all other elements are automatically seeking to attain when they undergo chemical change. It is by the group of the inert gases that the two chemically-opposite wings of the Periodic Classification are bound together and supported, much as the opposing halves of an arch require a keystone which abuts on them both.

(c) Lastly, there is this to observe. In the past ten years atomic weights have in some degree lost that primary significance, a belief in which first enabled the essential rhythms of the elements to be perceived. Although atomic mass (weight) may yet prove indispensable for proving atomic structure, the order of atomic weights in classification has been replaced by the almost parallel, but chemically more definite, Atomic Numbers. The sequence of these was originally laid down on purely chemical grounds; but the epoch-making work of the



Cambridge school of physicists has now given to that sequence a character of astonishing meaning and precision. In the light of all that has been done and is being done in the new branch of physical science—sub-atomics—the chemical Periodic Classification remains an indispensable guide, and it still presents a crucial problem.

### THE ION.

(1) **The Simple Theory.**—(a) The fundamental researches of Faraday, which made exact and greatly extended the knowledge obtained by Davy and others, gave the first clue to the way in which matter and electricity are connected. Without adhering to strictly historical sequence, we may review the salient facts.

Pure substances can be put into two classes according as they do or do not allow an electric current to pass through them easily. The really good conductors are all metals; the poor conductors and the insulators are non-metallic elements and most of the pure chemical compounds. The passage of a current through a metal—for instance, in an electric cable—makes no difference at all to the metal; it may grow warm at the time, owing to such resistance as it opposes, but otherwise it remains exactly in its initial state.

When *mixed* substances are studied, we find that there are three groups: one consists mainly of metallic alloys, such as brass, which conduct in just the same way as pure metals, showing no change thereby; and solutions, on the one hand conducting solutions and on the other non-conducting or insulating solutions.

Taking the two kinds of solutions, it turns out that there are many liquids, like chloroform, petrol, and others, which never yield conducting solutions, no matter what we dissolve in them; others, of which water is the chief, give conducting solutions, but only when the dissolved body is a compound and of a certain wide class of compounds. Thus, solutions of sugar

or of alcohol in water do not conduct; those of salts, of acids, of alkalis, do. Broadly speaking, those "organic" chemical substances which are not salts, acids, or alkalis are in the former class: all the rest, including the inorganic compounds, form electric conductors when dissolved in water.

We may now confine our attention to the conducting solutions. It is found that they differ profoundly from metals in the way in which they conduct the current; for, whereas metals are unaffected by the passage, these substances cannot conduct except in so far as they are decomposed chemically in the process. Where the current is led into the liquid, and where it leaves, there inevitably appear the separated components of the dissolved compound. Thus, when we pass a current through a solution of copper chloride, metallic copper is deposited at one side, chlorine gas is set free at the other, until all the copper chloride is gone—resolved into its two components. Using ordinary salt—sodium chloride—in solution, a similar thing happens; the sodium all goes towards one pole, the chlorine all goes towards the other; if we do not see metallic sodium as such at the appropriate pole, this is only because this particular metal happens to be unable to exist as such in the presence of water, which chemically attacks it; but its arrival is signalized by the production of hydrogen gas in its place, and the neighbouring part of the liquid is found to be rich in the piled-up sodium hydroxide simultaneously formed. By melting pure salt itself, we have a liquid which avoids this complication, and metallic sodium duly appears at one pole when the current passes.

In this way we can electrolyse, *i.e.*, resolve electrically, any member of this class of bodies which is soluble in water or other 'conducting solvent' (including the liquids which they themselves form when melted); every one is thus divisible experimentally into two opposite parts or 'radicles,' which are charged, when in solution, with positive and with negative electricity, according to which electric pole attracts them. Positive radicles turn out always to be the metallic constituents

of salts and alkalis and the hydrogen of acids; the negative radicle is what is formed by all the rest of a compound.

Here, then, is a puzzle, over a century old, which requires explaining: why should the metallic appearance of an element go hand in hand with its atoms being positively electrified when its compounds are dissolved in water or melted? The question will be answered a little later; the puzzle was not solved at all until a few years ago, and is still not perfectly cleared up.

(b) The next step is most concisely stated by using the molecular idea, and the atomic theory as it was sketched in the foregoing section. When a dissolved substance is decomposed at a pole in the act of allowing electricity to pass to that pole, it is fairly obvious that molecules must separately be partaking in this process; that is, we are watching on a large scale what is actually occurring to each separate molecule. Each single molecule of the substance, then, is to be thought of as having two parts, one of which is associated with positive electrification, the other with negative; in a molecule such as that of copper chloride, the copper atom becomes positive, and each of its two chlorine partners becomes negative. Arrived at the respective poles, these electrified atoms lose their charges and revert to the elementary state.

It is the electrified atoms or parts of a molecule which are called *ions*.

We thus gain the idea that the act of conduction is one of transport; that the atoms can serve as actual *vehicles* for electricity. What Faraday's experiments did was to show how much electricity the different atoms can carry; and, taking again the single illustration of copper chloride, we may put the result by saying that each of the two chlorine atoms takes one definite quantity of negative electricity, while the single copper atom takes exactly two such quantities of positive electricity. The molecule of copper chloride *as a whole*,  $\text{CuCl}_2$ , like copper chloride in bulk, is electrically neutral, the two negative charges of the chlorine ions being balanced by the double positive charge of the copper ion.



We have here the first indication of the unit of electricity, which was eventually to turn out to be the electron. Further, it became clear that the number of Faraday units of electricity (+ or -) which an atom can carry or unite with, is precisely the same as the number of chemical atoms which it can unite with; to put the matter shortly, its ionic charge is given by its chemical valency. This fact is at the basis of our ideas concerning the actual cause of chemical valency, and we must acknowledge Faraday as their founder.

For the present purpose, we have arrived at the view that the electrically neutral molecules of salts, either molten or in certain solutions, can divide into oppositely-electrified atoms or radicles which serve as the vehicles for transporting electricity when a current is turned on. We have still to learn whether the splitting is spontaneous, or whether it is merely local and transient and caused by the current which is applied: is it really inherent in the salts themselves? If we are to answer this logically we must seek evidence which is independent of electrical experiments; and such evidence we shall now review.

(c) In the section dealing with molecules we saw how it is possible to count the number of foreign particles of any kind which are mixed with those of a liquid to form a solution, by means of simple measurements such as those of boiling-point or freezing-point. These measurements have nothing to do with electrical properties as such.

When this method is applied to solutions in general—whether in water or not—it is found that those solutions which do *not* conduct electricity contain only so many foreign particles as have been put in, in the shape of molecules of dissolved material, when the solution was made up; but that, in the case of conducting solutions, in proportion as the solutions are good conductors, by so much are the foreign particles *more numerous* than the neutral molecules originally put in. Arrhenius, finding this, pointed out in 1887 the inevitable inference: that the extra number of foreign particles present in the

conducting solution must have been provided by the spontaneous breaking-up of the original molecules at the moment of dissolution; and that, when a current is afterwards switched on, it is these fragments which are alone instrumental in conveying the current. This, then, is the essence of Arrhenius' ionic theory of solution. Uniting the two quite independent lines of work which will always be associated with Faraday and with van 't Hoff, Arrhenius proved that Faraday's ions, or charged atoms and radicles, are not merely evanescent aspects of the electrical disruption of a chemical molecule, but are separately-existing though complementary objects; they are called into existence solely by the intrinsic nature of the parent molecule, and (as far as could then be told) by the medium in which that molecule is placed. The agency of an electric current has nothing to do with producing them, it only guides their movements.

(d) A third class of properties was at the same time brought into line; for it was shown that the faculty, peculiarly associated with salts, acids, and alkalis, of undergoing chemical changes instantaneously when mixed in solution, was to be connected with the proved dissociation of the molecules into ions. Further, a number of actions, classed under the heading of "catalysis," wherein the mere presence of one of these compounds is found to hurry on certain slow chemical changes suffered by organic compounds, were shown to be accounted for precisely if the *ions* of the catalysor, and not its unbroken molecules, are the effective accelerators.

(2) **Later Development.**—In these ways, three great groups of facts were correlated by one theory; and it might be said that for the remainder of the nineteenth century and well on into the twentieth, the phrase "physical chemist" meant chiefly one who was deducing or putting to the proof the consequences of the ionic theory. Out of an enormous mass of results we may select two additions which have been made to the simple original theory: these will now be outlined.

(a) Arrhenius' own followers did what disciples usually

do : they claimed more than he had, and neglected what virtues had lain in older views. But inevitably, the solid facts on which these older views had rested, however insecurely, were revived and were enlarged; and now, incorporated in the modern ionic theory of solution, they strengthen it exceedingly.

What was neglected in the first dawn of the theory was the fact that the liquid, in which the dissolved salt 'ionises,' plays a more than purely physical *rôle*; for, besides acting as a medium which allows the electrified opposites to separate, some of the solvent molecules unite chemically with the dissolved body or its ions. There is a vast literature of experiments and speculation on this, and it is far too technical a matter to enter upon here. All that can be said is, that chemical association with the solvent is inherent in the process of ionisation, and is probably a necessary factor in enabling the electrical charges to be comfortably accommodated upon the ions which are in this way enlarged. In many instances, the union with the solvent happens in a fashion so definite that there is no doubt whatever of its function as a factor which conditions ionisation. Such cases are examples of a general type of chemical union known to chemists as "co-ordination," whose study at the hands of Werner has provided one of the happiest conjunctions of organic with inorganic chemistry, by reason of the methods of investigation and because of the kind of atomic valency which is now found to be operative in the act of "co-ordination."

(b) The other outcome to be mentioned is more fundamental. It was recognised from the beginning that most salts are incompletely broken up into ions when they are dissolved; and yet there are many properties, clearly due to ions alone, which are also manifest in the substance when it is not in solution at all. For instance, the colours of many solid salts are exactly the same as those of the solutions in which they are present almost entirely as ions—permanganates, for example. Yet these salts do not conduct electricity when they are solid;



if we had to judge by this criterion alone, the solid crystals would have to be described as not ionised.

Here again, the methods of discovery are mainly too recondite to be followed unless one is a regular student of the subject; and once more we must adopt the unsatisfactory procedure of indicating results without adducing the facts which prove them.

Briefly then, the results are these: those substances which form freely-conducting solutions when they are dissolved, are ionised not only in solution but when they are in the pure solid state as well. Thus common salt crystals are made not of molecules of sodium chloride,  $\text{NaCl}$ , but essentially, of sodium ions ( $\text{Na}^+$ ) regularly intermeshed with chlorine ions ( $\text{Cl}^-$ ). The crystal does not conduct electricity, because every ion is rigidly confined and so none can migrate to the electric poles applied to the sides of the crystal. But at high temperatures, even before the crystal melts, the conduction increases, for the ions have been made more mobile.

When the solid salt is dissolved in water, or melted, or even vaporized at high temperature, many of these positive and negative ions move apart and become more or less independent, so that they are free to deliver up their electric charges separately at two poles, *i.e.*, to carry a current. According to the nature of the intervening medium, the purely electrical attraction of positive for negative—like for unlike—leaves many of the ions as only semi-detached pairs. The difference between 'good electrolytes' and poor electrolytes lies principally in the degree of this "semi-detachedness" when comparisons are made in similar surroundings; and, as has already been indicated, this degree is markedly influenced by the union which can occur with the molecules of the solvent itself.

The difference between electrolytes and non-electrolytes (including organic compounds) is helpfully thought of thus: that the latter consist of molecules in which either (i) the positive and negative ions present in the molecule are electrically attracted to each other too powerfully to be drawn

asunder, or (ii) the molecules do not contain distinct ions at all, but are groups of tightly-locked neutral atoms. It is not improbable that one and the same molecule can exhibit either of these two states as different phases of its existence; and that an input of energy—for example, the access of light to silver salts and to materials like luminous paint—can induce a more or less prolonged change from one phase to the other.

(c) Using the electron, the atom of negative electricity, we regard a positive ion as an initially neutral atom which has lost one or more of its constituent electrons, and a negative ion as an initially neutral atom which has gained these. In a salt crystal, the sodium atoms have each parted with an electron, becoming thereby positive sodium ions, and each dismissed electron has become attached to a chlorine atom, so constituting a negative chlorine ion. In pure metallic sodium the evidence is that here also each sodium atom has dismissed an electron, so becoming a positive sodium ion, just as in salt; but the separated electron finds no chlorine or other atom present which can carry it, and consequently it exists in the metal in a practically free state. Externally, a block of such metal is, of course, electrically neutral. But the lustre of a metal is rather the lustre of the electronic web; the metal's power of allowing electricity so freely to pass is due to the freedom with which the dotted tracery of electrons can sieve through the interstices of the coarsely-grained ionic lattice, which itself remains immobile; it is these free negative electrons which are shot forth when ultra-violet light falls on a metal and leaves it positively electrified; and, lastly, the chemical ability of a metal to dissolve in acid and form an ionized salt, depends on the fact that acids provide something positive (hydrogen ions) which can pair off with these negative electrons, to form neutral hydrogen and to leave the residual stack of metallic ions free to fall to pieces and pass as such into the liquid, where they remain.

(d) *Gaseous ions*.—The purely physical work initiated by Sir J. J. Thomson on the conduction of gases has disclosed facts

which have to do with the ions of salts. It appears that under the constraints of high electric tension, or of strong irradiation, or of the powerful bombardments of molecules at a high temperature, the molecules of elements in the gaseous form can form atoms which are electrically charged—that is, gaseous ions. The fate of the electrons which are missing from the atoms that have formed positive ions depends on circumstances; they may find other atoms to carry them, but failing that, they remain free, and this is the condition in a “vacuum tube,” or in the stars. Here our chief concern with the gaseous ions is, that the work which has to be expended in forming them by stripping off electrons from neutral atoms can be measured; and, as far as can be judged at present, it seems that these measurements of energy are throwing light on the chemical reactions which the atoms in question can undergo. For in such reactions the atoms use up or give out energy in being turned into ions and parts of chemical molecules; and the speed or slowness of their performances—which, incidentally, makes or mars a chemical industry—is primarily dependent on the supply of energy in some form to the molecules whose atoms and electrons are the active reagents.

The only illustration that will be given here of this very wide field of work is an extreme case: just as it has been found by the chemists that helium and its congeners are the most refractory of all elements to persuade into chemical activity, so it is found by the physicist that they of all gases have atoms the most difficult to ionize.

All this can be touched on and no more; it serves as one more reminder of the struts which span the fabric of physics and chemistry.

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## POSTSCRIPT

### PROFESSIONAL CHEMISTRY

THE excuse for the inclusion of this postscript in the present work is, that it presents what is certainly a "Phase in the growth of a science," to quote the sub-title of this book; for one of the objects of the book has been to set forth the reaction of human habits of mind upon the issues of the science itself, and there can be no doubt that the development of the study of science nowadays has brought in its train a set of educational and vocational conditions that have never arisen before, and that are inevitably reacting upon its further advancement. Those who are engaged in science are familiar with what is now to be said and may agree with it, but there may be other readers, and to these this postscript is more especially addressed.

The conditions referred to, peculiar to our day and doubtless permanent hereafter, are entailed by the simple fact of the great numbers of people who now practise and study some branch of physical science. It was guessed that the eighteenth century had a score or so of chemical practitioners for every one in the seventeenth; and the further increase which has taken place in the last few generations may be gauged when we learn that in the Journal of the Chemical Society the number of original communications has increased fifteenfold since the decade 1840-1850—about 400 papers now appear in it annually, each contributing new experimental material; and the number of Fellows is about 4000. This is for Britain and British countries alone; each other of several great nations has a similar tale to tell, and in the United States the American Chemical Society numbers some 17,000 members. The volume of periodical literature has increased proportionately, and the dissemination of new facts takes place with immeasurably greater speed than in the periods with whose chemical history we have been occupied; the circulation of new ideas is, of course, a process much less swift than this, and it still depends in a high degree upon personal missionary work. But, once an idea is mooted, the multiplicity of workers enables it to be

reviewed in all its bearings so as to be rejected, adopted, or modified; and thus a few years now can do what might have taken several generations a century or two ago; a hypothesis of any width has to be very well based if it is to stand up to the battering to which five or six years of the present day will submit it. For, although in the great number of investigators there may be a good many whose powers of criticism are not highly developed, apart from the leaders themselves there are numbers who are only too glad to be offered hypotheses the testing of which shall provide them with experimental work.

In the early part of this book it was remarked that the essence of the scientific habit of mind is in a continued readiness to be taught by external things and happenings. This deference to observation makes the scientific attitude a humble one, and none of the greatest men of science have been other than humble in the face of their own problems; if they have now and then been less docile in other matters, they have oftener been the more humanely temperate because they, above all, should know how hard it is to be sure, even among conditions which are subject to strict control. Moreover, in purely human affairs they find themselves confronted with facts in which a strictly scientific training has left them uninstructed, so that unless they have had or can acquire a knowledge of history and literature, they have no precedents at their service except those of their own personal human experience. The aforesaid spirit of deference, of accepting things as they are, tends to make a scientific man content when once he has traced a chain of causes and effects: when he has performed the impersonal work of experiment, inference, and testing, he has done his particular task and is free to straighten his back for a moment, to enjoy his small glimpse of cosmic harmonies, with a real artistic pleasure which he had been compelled to abjure while he was reaching them.

It is only rarely, or only incidentally, that discovery and invention—that is, the application of the discovery to material use—go equally together in one individual. It would be

fruitless to expect that discoverers shall be inventors or inventors discoverers; they follow complementary occupations, from civilization's standpoint, and each occupation is "a whole-time job." It is equally fallacious, but none the less common, to confuse scientific inventions with scientific discoveries by laying blame for harmful inventions at the door of the natural discoveries on which they were based. The march of pure knowledge in its many groups has been likened to the advance of allied armies, each sending forward its skirmishers, each busy in extending its front, in establishing liaison with others, and in consolidating ground already won; whilst the useful application of that knowledge, under the control of "*meum* and *tuum*, that great Rudder of humane affairs," is like the civil occupation which follows behind the army. And it is to be noted that the rate of progress of the civil occupation is limited by that of the army; and that an army marches on its stomach, even though it fights with its brains.

The last-mentioned fact is, of course, well and practically recognized in important quarters: the judicious endowment of academic research is a valued function of the Special Committee of the Privy Council (Department of Scientific and Industrial Research), whilst everywhere the large industries and important national developments now seek their expert advisers from among academically-trained men. The recent supremacy of German chemical industry was due largely to this, that the German firms made business men and technologists out of chemists; whereas, too often in the past, British firms tried to make chemists out of technologists and even out of business men.

On the educational side, the relation of a technological to a purely scientific training suffers still from a serious lay misapprehension, from which much harm can flow to persons, to universities, and to communities. The position is as follows:

If a man is to help in maintaining and improving the practice of any industry—if he is to be an "applied" scientist, a "chemical engineer," a "works physicist"—he *must* be able



to understand and to handle easily the very fundamentals and the latest knowledge of his science; he must come to his industry already provided not only with the innate and trained logic, but with the actual learning which is won in a full university curriculum of "pure" science, *plus*, for choice, a year or two of training in the methods of research in academic problems. For obviously, you cannot "apply" science until you have the science to apply. Nor is it of much use, if a man is to be a real agent of improvement in a manufacturing process, that he should have learnt merely the matter of a science, without its logic; a wide range of "potted knowledge" or book-learning may enable a man to listen to experts, but it does not enable him to be an expert. It is not for nothing that our largest manufacturing firms turn more and more to universities for first-class men to help with their larger technical enquiries. Now it is a rather curious fact that most of the older industries—those in which the practice has been evolved by "hit-or-miss" trials, or from some almost prehistoric accident—happen to involve processes of great scientific complexity. The sorts of industry in question include tanning, soil-cultivation, plant-breeding, brewing, baking, sugar-making, cotton and flax spinning, dyeing, gas and coke-making, iron-smelting, alloying, paint- and varnish-making, soap and tallow-making, building-material industries; and a great variety of others, all essential to material civilization. In all of these, because of the intricacy of the scientific principles involved, technical improvements have in the past had to depend upon trial-and-error observations conducted on the whole plant, in which any experiment is necessarily on a large and engineering scale. This method of enquiry, unless it has first received the sanction of the most exhaustive laboratory study of basic and of detailed factors, is excessively wasteful of time, materials, and money, and it often leads to no definite result. It is only recently that the progress of "pure" scientific knowledge has caught up with these old empirical manufactures, but it has practically done

so, and so it becomes essential for the scientific staff of each such manufacture to be in the forefront of learning and to have the ability to keep there; to know and to use in their laboratories the latest relevant academic discoveries; for otherwise their industry will remain stationary, be outstripped, and finally ousted. Proof of this can readily be found in the history of some of the manufactures already cited, and in the usage of the modern industries of electric lighting, photographic materials, alkalis, medical and surgical drugs, dye-making, and numerous others, which have set the pace in this country, in U.S.A., Germany, and other countries.

Now comes the educational difficulty. It will be admitted by any university teacher in this country that a goodly proportion of our educative labour is to this extent fruitless, that relatively few of our students who graduate in pure science are naturally fit to be original investigators, either academic or industrial, nor can training make them so. Those who *are* thus fit can usually be given the chance to prove it, for there is a fair distribution of temporary rewards for those who need them in their early postgraduate years, and there is for chemists also a good professional qualification—that of the Institute of Chemistry—which such men can obtain. Then a small proportion of these better men remain to take up university posts; most find their way into manufactures large or small, where on the whole they have a good chance to prove their worth and to use their gifts. It is upon these men that the progress of pure and applied science will come to depend, and they are an asset to the land when they remain at scientific work. But what about the others, the majority? These are the students who are the mediocre-to-poor Honours graduates of our modern universities; they include many of those who have been helped up the ladder with scholarships from school, and many of them have come from the professional classes to get a degree and, like their parents, to earn a professional living. With nine-tenths of them nowadays, the bread-and-butter view of science

is first and foremost, not unnaturally; to them science is less an education than a means to an end, a key to a salary and a business rather than a vocation. When we hear of "the glut of young chemists," it is this body that is meant; and their numbers are increased in Britain every year by many hundreds in chemistry alone, to say nothing of the less-frequented branches of science.

This situation is not, of course, peculiar to students of science; but it seems worth while to make the point that at least a part of the difficulty arises, in science, from a misapprehension that could be corrected. Too often these young men and women whose ability as scientists is no more than ordinary, have been put into science by their parents under the mistaken impression that the letters "B.Sc." are as sure a passport to an income as the qualifications "M.B., B.S.", whilst they can be earned at three-fifths the cost of a medical degree. But the point cannot be too strongly insisted upon that a university curriculum in science cannot equip a man as a professional *scientist* unless he happens to belong to the first class. The rest are on the most precarious ground; for they cannot live on in university posts, and when they do find positions as minor assistant chemists in industries, their powers as investigators are not usually worth to their employers and the public more than a pittance, entailing years of life on a lowly scale.

There are three classes of university graduate in science: one, whose gifts make scientific investigation, either academic or technical, his proper life-work; a second sort understands the lore that he has been taught, and is really interested in it, but he has not that particular and inborn turn of mind which makes a laboratory-researcher; while the third sort takes his science as being not unusually interesting, and feels that if others will do the discovering, he can turn it to use.

Now, for the first sort, a university training in science amounts to a professional qualification, and he is safely provided for. If his desire is towards industry but he wishes for further training before actually seeking a post, he can at some



universities take up a diploma-course in the principles of applied chemistry; or he may decide to enter the biological field as a bio-chemist or a bio-physicist; all these are *post*-graduate matters. For the second sort, the science degree is necessary but is not all that he needs, for it is from this category of men and women that our schools most largely draw their teachers. And this second sort is likely to make a really good teacher if he has the personal essentials; but to be eligible for the greatest number of school posts he will be required to supplement his science course with a year or two of training in the principles of teaching. *Then* he has his professional qualification, and only then. It is obviously very important for national education that he shall have gained in his science course a sound attitude to his subjects; and a very heavy responsibility is thus put upon university lecturers, who have to try to imbue their students, the future school teachers, with the spirit as well as with the facts of the science. And the lecturer is handicapped by the necessity of covering an always-increasing syllabus of facts in a rarely-increasing period of time.

Of the third sort it can unhesitatingly be said that a course of pure science was the wrong university training for him, and he ought to have been deflected from it at the outset, from a knowledge of his performances in the last years of school. (This may sound very obvious; but the actual state of affairs shows that even if it is obvious, it is far too often not acted upon.) It is from men whose lack of the special instinct for pure science would put them in this category that are drawn some of our finest physicians and surgeons, and some of our great engineers: if then, a student of this group wishes to go to a university, let him enter from the start as a medical or as an engineering student, and he will have the chance to hold his own, and more, with his scientific friends. And—let it be emphasized once more—if a man in this category decides instead to devote all his training to some avowedly “technological science” course, which he may find in some universities, then he must not expect it to bring him more than a meagre

livelihood, for it is from the ranks of first-rate scientific graduates that modern technologists are drawn.

The writer has ventured upon the foregoing remarks not only in the hope that they may be helpful to prospective students of science, but because the situation which they serve to exhibit is worth recording as the phase of science in which its devotees have had to abandon the status of amateurs for that of professionals. Whether science itself thereby gains or loses is yet to be seen, but it can hardly lose, for its real leaders must always be amateurs at heart. That applied science must gain and not lose is unquestionable.

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